

AMERICAN
POLITICAL
SCIENCE
REVIEW

JUNE 1990
VOLUME 84 NUMBER 2

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AMERICAN POLITICAL SCIENCE REVIEW

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QUARTERLY JOURNAL OF THE
AMERICAN POLITICAL SCIENCE ASSOCIATION

JUNE 1990
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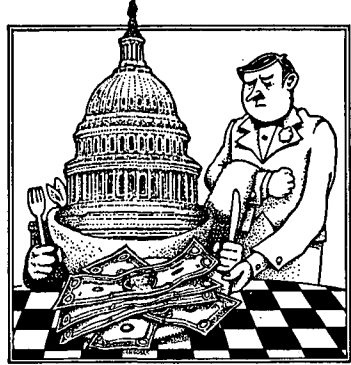
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ARTICLES

RACE, SOCIOPOLITICAL PARTICIPATION, AND BLACK EMPOWERMENT

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Using 1987 national sample survey data that included a large black oversample, we reexamine black-white differences in sociopolitical participation. We hypothesized that increases in black empowerment would affect the level of black sociopolitical participation and change the nature of black-white differences in political behavior. The results show that blacks in high-black-empowerment areas—as indicated by control of the mayor's office—are more active than either blacks living in low-empowerment areas or their white counterparts of comparable socioeconomic status. Furthermore, the results show that empowerment influences black participation by contributing to a more trusting and efficacious orientation to politics and by greatly increasing black attentiveness to political affairs. We discuss the results' implications for theoretical interpretations of when and why black sociopolitical behavior differs from that of whites.

In the early 1970s students of black political behavior reached consensus on two points: (1) that blacks tended to participate more than whites when differences in socioeconomic status were taken into account (Milbrath and Goel 1977; Olsen 1970; Verba and Nie 1972) and (2) that a strong sense of "ethnic community," or group consciousness, was the stimulus to heightened black participation (Guttorbuck and London 1983; Shingles 1981; Verba and Nie 1972). Much of the data supporting this view of black political behavior comes from the late 1950s and 1960s. Over the ensuing 20 to 30 years, however, blacks made enormous strides in socioeconomic status and political influence. For example, Williams and Morris (1987, 137) recently reported that the number of black elected officials

rose from "fewer than 103 in 1964 to 6,384 in 1986." As a result of such changes it is appropriate to reexamine theoretical and empirical notions about black-white differences in sociopolitical behavior.

It is plausible to speak of substantial—though far from ideal (Jennings 1984)—"black political empowerment." Increases in the control of institutionalized power by blacks is likely to have considerable impact on the level and nature of black sociopolitical behavior. Our purpose is to develop and test theoretical ideas about the effects of empowerment on racial differences in participation. The research is based on data from the 1987 General Social Survey, which involved a national probability sample of adults, a large black oversample (total black $N = 544$), and a replication of the battery of sociopolitical

behavior measures from Verba and Nie's (1972) classic study.

Background

Patterns and Theories of Black Participation

Previous attempts to explain black-white differences in participation have focused on sociodemographic, psychological, and structural factors. Some of the earliest research explained racial differences in political behavior on the basis of blacks' lower average levels of education, occupational status, and income (Matthews and Prothro 1966; Orum 1966). This approach is commonly known as the "standard socioeconomic model" (Verba and Nie 1972) and is a baseline model for most research on participation. This model, however, was confounded by the frequent empirical finding that once controls for socioeconomic status were introduced, blacks actually participated at higher rates than whites.

Two psychological theories have been advanced to explain this pattern. The first, compensatory theory, grows out of a more sociological tradition concerned with both political and social participation. This view posits that blacks join organizations and become politically active to an exaggerated degree in order to overcome the exclusion and feelings of inferiority forced on them by a hostile white society (Babchuk and Thompson 1962; Myrdal 1944; Orum 1966).

The second theory, the ethnic community approach, holds that membership in disadvantaged minority communities leads people to develop strong feelings of group attachment and group consciousness. One product of these feelings is the emergence of group norms that call for political action to improve the status of the group (Antunes and Gaitz 1975; Miller et al 1981; Olsen 1970; Verba and Nie 1972). In particular, very high levels

of participation were found for blacks who exhibited a political orientation characterized by low levels of trust in government and high levels of personal efficacy (Guterbock and London 1983; Shingles 1981). The extraordinary levels of participation among blacks, then, reflected mainly the actions of the "politically discontented" among them who were acting on community norms.

Tests of these theories have suffered from several methodological limitations (Walton 1985, 78-82). These shortcomings include small numbers of black respondents (e.g., McPherson 1977; Olsen 1970; Orum 1966) and indirect measures of central concepts (e.g., Verba and Nie's group consciousness measure). Even the most recent tests of these ideas are based on data gathered in the 1960s (Guterbock and London 1983; Shingles 1981). More troubling at a conceptual level is the fact that these theories were designed to explain black sociopolitical behavior at a time when blacks were struggling for basic inclusion in U.S. society and politics. Profound changes in the social and political status of blacks, however, call into question the applicability of such theories. Specifically, what should we expect about contemporary racial differences in political behavior? Should we expect, for example, that the politically discontented continue as the most active group of blacks?

Black Political Empowerment

Understanding black participation in the contemporary period, we believe, requires taking into account the likely effects of black political empowerment. By political empowerment—or political incorporation, as some have called it (Browning, Marshall, and Tabb 1984)—we mean the extent to which a group has achieved significant representation and influence in political decision making.

Black Empowerment

The business of U.S. politics is transacted on several levels. Black gains in public officeholding, however, have primarily been at the state and local levels (Joint Center for Political Studies 1988, 8). Blacks have made tremendous strides in obtaining seats in state legislatures and on city councils and school boards (p. 13). The most notable black gains, we believe, have been at the mayoral level (see Persons 1987). In major cities such as Atlanta, Detroit, Gary, Los Angeles, and others, black mayors have controlled city hall for more than a decade. Conceptually, we focus on whether blacks have captured the mayor's office because it involves the highest degree of local empowerment, usually signaling both a high level of organization among elites in the African-American community and a relatively high degree of control over local decision making (Browning, Marshall, and Tabb 1984; Nelson and Meranto 1977).

There are two interrelated reasons why such empowerment should influence mass sociopolitical participation. First, empowerment should influence participation because sociopolitical behavior has a heavily instrumental basis. Like Wolfinger and Rosenstone (1980), we believe that people participate because the perceived benefits of doing so outweigh the perceived costs. Second, empowerment should influence participation because macro level aspects of a person's sociopolitical environment affect cost-benefit calculations. There is a large literature on the effects of political contexts on cost-benefit calculations relevant to participation. Studies have emphasized legal factors (Ashenfelter and Kelley 1975; Wolfinger and Rosenstone 1980), electoral factors (Gilliam 1985; Patterson and Caldeira 1983), organizational factors such as mobilization efforts by political parties (Flanigan and Zingale 1979; Key 1949), and cues from political figures indicating likely policy responsiveness

(Bullock 1981; Whitby 1987). Our primary interest is in this latter type of contextual influence. We hypothesize that where blacks hold more positions of authority, wield political power, and have done so for longer periods of time, greater numbers of blacks should see value in sociopolitical involvement.

We expect, then, that the greater the level of empowerment, the more likely it is that blacks will become politically involved (Hamilton 1986). Empowerment should increase participation because of its effects on several social psychological factors, in particular, its impact on levels of political trust, efficacy, and knowledge about politics. Blacks in high empowerment areas should feel more trusting of government, express higher levels of efficacy, and become more knowledgeable about politics than blacks in low-empowerment areas. All of which should, in turn, contribute to higher levels of participation.

The impact of empowerment on levels of trust and efficacy among blacks should also change the nature of *black-white differences* in the extent and correlates of participation. In areas of high black empowerment, blacks should participate at rates equal to, or greater than, whites (all other things being equal). In areas of relatively low black empowerment, blacks should participate at rates lower than whites. Furthermore, black empowerment should bring greater similarity between blacks and whites in the relationship of political orientations to sociopolitical participation. Earlier research found that the most active blacks were politically discontented; that is, they exhibited a combination of low levels of trust in government and high levels of personal political efficacy. In contrast, the most active whites were found among those aptly labeled "politically engaged"—those individuals with high levels of trust and high levels of efficacy. Growing black empowerment suggests a shift of the most ac-

tive blacks to the same type of "engaged" orientation of the most active whites.

Data and Measures

The data come from the National Opinion Research Center's 1987 General Social Survey (GSS; Davis and Smith 1987). This is a nationally representative multi-stage probability sample of English-speaking adults living in the continental United States. The main GSS sample included a total of 1,466 respondents, with 191 blacks, 1,222 whites, and 53 nonblack nonwhites (excluded from all analyses), with an overall response rate of 75.4%. The 1987 GSS also includes a large black oversample ($N = 353$). The oversample had a response rate of 79.9% and brings the total black sample size to 544. There were no statistically discernable differences in age, sex, education, family income, occupational prestige, or regional distribution between blacks in the oversample and blacks in the main GSS.

Level of Empowerment. Respondents from primary sampling units (PSUs) where the largest city had a black mayor at the time of the survey are scored as living in high-black-empowerment areas. Individuals living in PSUs without a black mayor or in PSUs with black mayors only in a smaller city, were scored as low-black-empowerment areas.¹

Sociopolitical Participation. Since individuals may act to influence political decision making significantly through means other than voting, our research focuses on broad measures of sociopolitical involvement; that is, our goal is to understand general patterns of participation, not merely electoral turnout. Fortunately, the 1987 GSS replicated the large battery of sociopolitical behavior measures developed by Verba and Nie (1972). Full wording for the 15 individual participation measures is shown in the

Appendix. Verba and Nie found that these 15 indicators reflected four major modes of sociopolitical involvement: voting, campaigning, communal activity (membership in groups and organizations that work to solve problems of broad social importance), and particularized contacting (direct contact concerning a personal matter with an elected official). We find a similar factor structure for the 1987 data and thus employ scales for these four major modes of participation, as well as a summary participation index, as our main dependent variables. For most analyses we rely on the summary participation index (we note differences across the modes when relevant). Details on scale reliability and index construction are reported in the Appendix.

Analysis and Results

Previous research suggests that blacks should participate at lower rates than whites but that this pattern is reversed after introducing controls for socioeconomic status. The results presented in Tables 1 and 2 confirm the first of these expectations and disconfirm the latter. Whether examining the individual indicators of participation (Table 1) or the scaled measures of voting, campaigning, communal involvement, and particularized contacting (Table 2), blacks generally participate at lower rates than do whites. Of the 15 individual measures 14 show that whites are more active than blacks, with eight of these differences reaching statistical significance. Blacks are consistently less active than whites on the measures of voting. The black-white differences tend to be smaller for campaign involvement, which is usually regarded as a high-initiative political behavior.

Blacks have lower participation scores than whites on all of the major modes of participation scales—voting, campaigning, communal activity, and particular-

Black Empowerment

**Table 1. Race Differences in Sociopolitical Participation:
Participation Items by Race**

Participation Items	Blacks (%)	Whites (%)	Difference (%)
Voting			
Local voting	76 (534)	82 (1,208)	-6%**
1980 national election	65 (512)	70 (1,187)	-5%*
1984 national election	61 (513)	71 (1,187)	-10%**
Campaigning			
Persuade others	42 (539)	48 (1,215)	-6%
Donate money	16 (539)	24 (1,215)	-8%**
Attend meeting	22 (540)	19 (1,214)	3%
Work	21 (540)	28 (1,217)	-7%*
Member political club	3 (544)	4 (1,222)	-1%
Communal activity			
Local problem solving	32 (538)	34 (1,219)	-2%
Start local group	16 (536)	18 (1,218)	-2%
Contact local officials	17 (539)	26 (1,215)	-9%**
Contact nonlocal officials	10 (539)	25 (1,222)	-15%**
Community group activity	39 (544)	42 (1,222)	-3%
Particularized contacting			
Contact local officials	8 (539)	10 (1,214)	-2%
Contact nonlocal officials	5 (539)	8 (1,215)	-3*

Note: Base numbers of cases in parentheses.

* $p < .05$.

** $p < .001$.

**Table 2. Race Differences in Sociopolitical Participation:
Participation Scales by Race**

Modes of Participation Scales	Unadjusted Means		Adjusted Means	
	Blacks	Whites	Blacks	Whites
Voting	-16.59	3.64**	2.69	-.39
Campaigning	-8.71	1.35	5.37	-.79
Communal activity	-16.38	2.56**	4.58	-.67
Particularized contacting	-10.75	1.67*	-3.70	.70
Summary index	-18.40	2.86**	4.86	-.71

Note: Adjusted means are corrected for socioeconomic status (education, occupational prestige, and family income) and demographic factors (age and sex).

* $p < .05$.

** $p < .001$.

ized contacting—as well as on the summary participation index. (The scales are scored to have a mean of approximately zero, hence negative scores indicate below-average rates of participation). Four of these five comparisons show a significant difference favoring whites, with the campaigning scale providing the only exception. The last two columns of Table 2 report mean participation scores after adjusting the scales for the respondents' education, occupation, family income, age, and sex.² None of the adjusted scale comparisons show a significant black-white difference, indicating that the compositional differences in socioeconomic status—especially education—accounts for lower average black participation.³

Similar black-white rates of participation, net of socioeconomic and demographic factors, still leave open the possibility of empowerment effects on participation. We hypothesized that the level of black sociopolitical involvement would be higher in areas where blacks were politically empowered and that racial differences would favor blacks over whites in high-black-empowerment areas. The results reported in Table 3 strongly support these hypotheses. The top half of the table shows that blacks in high-empowerment areas are significantly more active (32.65) than blacks in low-empowerment

areas. In addition, blacks in low-empowerment areas are significantly less active than comparable whites (-33.96). The black-white difference in high-empowerment areas favors blacks but does not reach statistical significance. Most important, as the bottom half shows, after we adjust the summary participation index for socioeconomic status, age, and sex, blacks in high-empowerment areas are indeed more active than comparable whites.⁴ There is no racial difference in participation, net of socioeconomic status, age, and sex, in low-empowerment areas.

We have argued that empowerment influences black participation because it is a contextual cue of likely policy responsiveness to black concerns. If so, the empowerment effect on participation should work through those psychological factors that facilitate political involvement; that is, level of empowerment should influence participation because it increases attentiveness to politics among blacks as well as increasing their levels of political trust and efficacy. The results shown in Table 4, which presents mean item and scale scores for the measures of political knowledge, trust, and efficacy, unequivocally support the first of these hypotheses but speak equivocally regarding the impact of empowerment on levels of trust and ef-

Black Empowerment

Table 3. Black Political Empowerment, Race, and Participation

Race	Mean Participation Score, Level of Black Empowerment		
	Low	High	Difference
Summary Participation Index			
Blacks	-29.26 (358)	3.39 (182)	-32.65***
Whites	4.70 (1,047)	-8.64 (170)	13.34
Difference	-33.96***	12.03	
Adjusted Summary Participation Index			
Blacks	-2.05 (294)	17.90 (159)	-19.95*
Whites	1.19 (930)	-12.45 (152)	13.64
Difference	-3.34	30.35**	

* $p < .05$.

** $p < .01$.

*** $p < .001$.

ficacy. In percentage terms, blacks living in high-empowerment areas are significantly more likely to name the local school board president correctly (38% vs. 25%), their representative (31% vs. 22%), and the state governor (75% vs. 61%) than blacks living in low-empowerment areas.⁵ The impact of empowerment on trust and efficacy appears to be localized in nature. Blacks in high-empowerment areas trust local officials more and expect to have greater influence with them than do blacks in low-empowerment areas. However (not too surprisingly), the sort of "local" empowerment we tap here does not change trust in the federal government or improve blacks' views of the motivations of politicians in general.

These results also illuminate black-white differences in participation. Whites in high-black-empowerment areas are often less politically knowledgeable than whites in low-black-empowerment areas. Specifically, among whites, those living in high-black-empowerment areas were significantly less able to name the local

school board president (14% vs. 34%) or their representative (29% vs. 41%) but were just as able to name the state governor (80% vs. 82%). The level of black-empowerment does not, however, consistently influence whites' feelings of trust and efficacy. In sum, whites tend to pay less attention to local politics when blacks control local offices but do not become generally less trusting or efficacious as a result.

We can gain greater leverage on how empowerment affects behavior by considering whether it also helps shape basic political orientations involving the intersection of political trust and efficacy (Gilliam and Bobo 1988). Previous research found that the most active blacks were politically discontented—those who combined high feelings of efficacy with low feelings of trust—but that the most active whites were highly efficacious and highly trusting. We hypothesized that in the modern period the most active blacks should maintain the same sort of "engaged"—high efficacy and high trust—

**Table 4. Mean Political Knowledge, Trust, and Efficacy
by Black Political Empowerment and Race**

Knowledge, Trust, and Efficacy	Empowerment Level			
	Blacks		Whites	
	Low	High	Low	High
Correctly name school board head	1.25 ^a	1.38	1.35 ^b	1.14
Correctly name congressman	1.22 ^a	1.31	1.41 ^b	1.29 ^c
Correctly name governor	1.61 ^a	1.75	1.81	1.79 ^c
Political knowledge scale	4.07 ^a	4.44	4.57 ^b	4.22 ^c
Local trust ^d	2.33 ^a	2.50	2.68	2.74 ^c
Federal trust ^d	2.10	2.10	2.36 ^b	2.49 ^c
Political trust scale	2.22	2.30	2.52	2.62 ^c
Local influence ^e	2.26 ^a	2.48	2.51	2.55 ^c
Local officials care ^e	2.88	2.83	3.03 ^b	2.90 ^c
Political efficacy scale	2.57	2.65	2.77	2.72 ^c

^aSignificant difference by empowerment among blacks.

^bSignificant difference by empowerment among whites.

^cSignificant difference between blacks and whites on item or scale.

^dThe two political trust items are modestly correlated ($r = .36, p < .001$) and are worded as follows. Local trust: "How much of the time do you think you can trust the local government here in [respondent's local government unit] to do what is right—just about always, most of the time, only some of the time, or almost never?" Federal trust: "How much of the time do you think you can trust the government in Washington to do what is right—just about always, most of the time, only some of the time, or almost never?"

^eThe two political efficacy items are modestly correlated ($r = .39, p < .001$) and are worded as follows. Local influence: "How much influence do you think people like you can have over local government decisions—a lot, a moderate amount, a little, or none at all?" Local officials care: "If you had a complaint about a local government activity and took that complaint to a member of the local government council, would you expect him or her to pay a lot of attention to what you say, some attention, very little attention, or none at all?" Scoring of all items has been reversed for these analyses.

orientation characteristic of politically active whites and do so, in part, because of improved levels of black political empowerment. In the main, the results of Table 5 support these hypotheses.⁶ To be sure, blacks are less likely than whites (25% vs. 42%) to fall into the politically engaged group (rows 1-2) and are more likely than whites to appear among the alienated who lack in both trust and efficacy (32% vs. 19%). However, rows 3-6 show that blacks in high-empowerment areas are more likely to be among the politically engaged than blacks in low-empowerment areas. Moreover, rows 7-10 show that politically engaged blacks are the most active segment of the black community. Indeed, after we have controlled for socioeconomic status, age, and

sex, politically engaged blacks score as more active than politically engaged whites. This is a sharp reversal of the patterns for blacks found in earlier research.⁷

Empowerment increases black participation. It appears to do so because it increases attentiveness to politics and because it contributes to a more engaged orientation to politics. Still unclear is whether empowerment exerts direct effects on participation, or works largely through its impact on knowledge and political orientation. Table 6 reports the results of Ordinary Least Squares (OLS) regression analyses of participation, which indicate that among blacks the effect of empowerment on participation is mediated modestly by its influence on political orientations and powerfully by

Black Empowerment

**Table 5. Political Orientation, Empowerment, and Participation
among U.S. Blacks and Whites, 1987**

Variables by Race	Politically Engaged	Politically Discontented	Politically Obedient	Politically Alienated	Total	Number of Cases	f-ratio	d.f.
Political orientation (%)								
$(\chi^2 = 70.87, \text{d.f.} = 3, p < .001)$								
Blacks	25	20	22	32	100	531	—	—
Whites	42	12	26	19	99	1,208	—	—
Black empowerment (%)								
Blacks $(\chi^2 = 9.71, \text{d.f.} = 3, p < .05)$								
Low	22	21	21	36	100	350	—	—
High	32	18	24	26	100	181	—	—
Whites $(\chi^2 = 3.41, \text{d.f.} = 3, \text{n.s.})$								
Low	41	13	26	20	100	1,038	—	—
High	44	9	29	18	100	170	—	—
Participation^a								
Unadjusted								
Blacks	17.40	-13.35	-37.33	-33.21	—	530	10.29	3/526
Whites	33.51	32.19	-34.12	-27.48	—	1,206	45.69	3/1,202
Adjusted								
Blacks	39.37	3.03	-5.55	-12.87	—	444	9.42	3/443
Whites	20.44	25.90	-26.58	-26.24	—	1,074	27.04	3/1,070

^aSummary participation index means.

its influence on political knowledge. Model 1 shows that among blacks empowerment has a significant positive effect on participation, net of education, family income, occupation, age, sex, region of residence, and "urbanicity." (We added region and "urbanicity" to ensure that the empowerment effect was not merely capturing north-vs.-south or size-of-place effects.) However, model 2 shows that by adding a series of dummy variables representing political orientations, the coefficient for empowerment remains significant even though it decreases by about 18%, from 23.31 to 19.02. On adding political knowledge to the model (model 3), the coefficient for empowerment is cut by more than half and becomes insignificant. Hence, all of the impact of empowerment on participation is indirect via its tendency to encourage more engaged political orientations and greater attentiveness to politics among blacks.⁸ We should note that political

orientations bring a significant increase in variance explained (3%), with the politically engaged blacks scoring as significantly more active than the politically discontented. Furthermore, the political knowledge scale brings a substantial increase in variance explained (12%) and has the single strongest direct effect (partial beta = .38, $p < .001$) on summary participation scores among blacks.

The final column of Table 6 shows that empowerment has no effect on white participation; but, as with blacks, political orientations and political knowledge strongly influence levels of participation. We should note that political knowledge has a stronger effect on participation among blacks than it does among whites. Viewed in this light, the effect of empowerment on black levels of political knowledge is of signal importance.

A more concrete view of rates of participation can be seen in Table 7, which shows predicted participation scores for

blacks and whites under the OLS models in columns 3 and 4, respectively. First, blacks in high-empowerment areas are more active than those in low-empowerment areas regardless of political orientation. Second, blacks in high-empowerment areas are more active than their white counterparts among the engaged, among the obedient, and among the alienated. Only among the politically discon-

tented, once regarded as the most politically active segment of the black community, do blacks in high-empowerment areas participate at lower rates than comparable whites. This suggests that otherwise efficacious blacks who do not trust black politicians in their communities are especially likely, relative to similar whites, to withdraw from politics. Third, blacks in low-black-empowerment areas,

Table 6. Regression Models of Summary Participation Index

Independent Variables	Blacks			Whites
	Model 1	Model 2	Model 3	
Constant	-265.63***	-250.89***	-331.83***	-283.02***
Variables				
Empowerment	23.31** (.12)	19.02* (.10)	9.14 (.05)	-4.66 (-.01)
Political orientations				
Engaged	—	31.63** (.15)	31.89** (.15)	-.53 (-.00)
Obedient	—	-10.36 (-.05)	-7.96 (-.03)	-45.81*** (-.19)
Alienated	—	-16.88 (-.08)	-18.19 (-.09)	-45.49*** (-.18)
Political knowledge	—	—	36.49 (.38)	25.26*** (.24)
Social background				
Age	1.84*** (.33)	1.78*** (.32)	1.24*** (.22)	1.26*** (.21)
Education	10.89*** (.39)	10.17*** (.36)	7.13*** (.25)	8.04*** (.24)
Occupation	.78* (.11)	.68* (.09)	.38 (.05)	.65** (.09)
Family Income	.01 (.00)	.01 (.00)	-.08 (-.01)	.30 (.05)
Region (South = 1)	15.54 (.08)	16.82* (.09)	9.25 (.05)	-4.52 (-.02)
Sex (female = 1)	1.60 (.01)	-.04 (-.00)	-5.35 (-.03)	5.04 (.02)
"Urbanicity"	-.07 (-.00)	.10 (.00)	.92 (.02)	-.08 (.00)
Adjusted R ²	.22	.25	.37	.30
Number of Cases	437	437	437	1,040

* $p < .05$, two-tailed test.
 ** $p < .01$, two-tailed test.
 *** $p < .001$, two-tailed test.

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Table 7. Predicted Mean Participation Scores by Race, Empowerment, and Political Orientation

Political Orientation	Empowerment Levels			
	Blacks		Whites	
	Low	High	Low	High
Politically engaged	18.11	36.59	38.25	20.03
Politically discontented	-21.29	24.50	33.77	35.91
Politically obedient	-43.99	-21.02	-27.50	-46.22
Politically alienated	-49.50	-3.10	-31.96	-37.75

regardless of orientation, tend to be less active than whites in the same areas.

Discussion and Conclusions

Our results show, first, that where blacks hold positions of political power, they are more active and participate at higher rates than whites of comparable socioeconomic status. Second, black empowerment is a contextual cue of likely policy responsiveness that encourages blacks to feel that participation has intrinsic value. This conclusion is based on the finding that empowerment leads to higher levels of political knowledge and that it leads to a more engaged (i.e., trusting and efficacious) orientation to politics.

An alternative interpretation of these results holds that black empowerment is the outcome of higher participation brought about by registration and turnout drives when a viable black candidate emerges. This explanation of our results is unconvincing on logical and empirical grounds even though we agree that the mobilization of black voters is a necessary component of the accomplishment of empowerment (Browning, Marshall, and Tabb 1984). First, blacks are not newcomers to elective office in most of the "empowered" areas in our sample, and our dependent variables are general patterns of individual behavior. Hence, it is unlikely that we have found merely the short-term effects of black voter mobiliza-

tion efforts. What is more, the effects of empowerment are not restricted to electoral turnout. Second, if the association between empowerment and participation were merely the result of voter mobilization drives by black candidates, we should have found strong direct effects of empowerment on participation among blacks. Instead, the data show that empowerment works through the psychological factors of political orientation and (especially) level of actual political knowledge. We suggest that black empowerment, whatever heightened mobilization this feat initially requires, has broad and lasting consequences on how often, and why, blacks become active participants in the political process. One sign of the potential for such effects is that whites, too, are affected. Recall the finding that whites in high-black-empowerment areas are less politically knowledgeable than whites in low-black-empowerment areas.

These results call for changes in our empirical and theoretical ideas about black sociopolitical behavior. Studies of sociopolitical participation based on data from the late 1950s and into the 1960s found that blacks participated less than whites, that blacks were more active than whites at any given level of socioeconomic status, and that greater black involvement was rooted in group consciousness and a sense of political discontent. Substantively, these patterns were correctly read as showing that (1) blacks were fighting for

basic civic inclusion and to obtain the larger goal of improving the material status of the group and (2) that full understanding of patterns of sociopolitical participation in the United States required one to take race into account.

The significance of race for sociopolitical behavior has evidently changed. On the one hand, we find that blacks generally participate at the same rate as whites of comparable socioeconomic status and that the politically engaged are the most active segment of both groups. It is tempting to conclude, therefore, that the importance of race for patterns of sociopolitical participation has greatly declined. On the other hand, blacks are more active than comparable whites in areas of high black political empowerment. In addition, level of empowerment shapes both blacks' likelihood of adopting an "engaged" orientation to politics and their basic levels of knowledge about political affairs. These psychological orientations to politics, in turn, powerfully affect a person's level of sociopolitical involvement. It is more accurate, then, to conclude that race now shapes sociopolitical behavior in different ways and for somewhat different reasons than held in the past.

In our judgment, these differences reflect broad legal-political-economic changes that improved the general social standing of many blacks and, most directly, brought a tremendous increase in the number and influence of black elected officials. To be sure, the core political goals of blacks have steadily been full and fair inclusion in all domains of U.S. society (Hamilton 1984; Jones 1972; Walton 1985). When the pathways to these objectives were fundamentally blocked, different strategies and orientations were necessary than now seem appropriate in a context of significant wielding of institutional power by blacks. With the goal of basic civic inclusion largely accomplished, the black political agenda has shifted to the goal of maintaining, exploiting, and ex-

panding the political and economic resources available to the black community (Hamilton 1986).

Nonetheless, the degree of black political empowerment and general social progress must be kept in perspective. Blacks gained control of mayoral offices at a time when the power of urban political machines continued to decline, when population and commerce were shifting to suburban areas (Wilson 1980), and when federal programs became less generous (Moore 1988). Hindrances to black empowerment in the form of cumbersome voter registration procedures, district boundaries that dilute the black vote, gerrymandering, hostility to black candidates among a significant number of whites (Williams and Morris 1987), and the cooptation of some black leaders (Browning, Marshall, and Tabb 1984; Jennings 1984) are still problematic. In addition, the persisting social segregation and economic disadvantages of blacks (Farley and Allen 1987; Wilson 1987) constitute structural bases for black racial identity formation (Allen, Dawson, and Brown 1989) as well as for sharp black-white political polarization over race relations issues (Bobo 1988), social welfare policy attitudes (Bobo n.d.; Gilliam and Whitby 1989), and basic life satisfaction (Thomas and Hughes 1986). Black progress and political empowerment are still partial and incomplete even though they have advanced far enough to affect how often, and why, blacks become politically active.

Further investigations of changing black sociopolitical behavior and the influence of black empowerment will require studies with larger samples of blacks. Future research should develop direct indicators of whether black respondents think black officials are more responsive to their needs than white officials (Jackson and Oliver 1988). Full exploration of these ideas will require data on whether black officials have the inclin-

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ation and resources to produce desired outcomes for their constituents (Eisinger 1982). Tapping the reactions of whites to black elected officials is a necessary component of this research. In addition, the empowerment model may be extended to other U.S. minority groups. The growing electoral power of Latinos, for example, might be fruitfully studied within the empowerment framework we developed here.

Appendix

Wording of Participation Items

Voting. (1) "Now in 1980, you remember that Carter ran for President on the Democratic ticket against Reagan for the Republicans, and Anderson as an independent. Do you remember for sure whether or not you voted in that election?" (2) "In 1984, you remember that Mondale ran for President on the Democratic ticket against Reagan for the Republicans. Do you remember for sure whether or not you voted in that election?" (3) "What about local elections—do you always vote in those, do you sometimes miss one, or do you rarely vote, or do you never vote?"

Campaigning. (1) "During elections do you ever try to show people why they should vote for one of the parties or candidates? Do you do that often, sometimes, rarely or never?" (2) "In the past three or four years, have you contributed money to a political party or candidate or to any other political cause?" (3) "In the past three or four years have you attended any political meetings or rallies?" (4) "Have you done work for one of the parties or candidates in most elections, some elections, only a few, or have you never done such work?" (5) "Would you tell me whether or not you are a member of political clubs? [If yes,] Do political clubs to which you belong do anything to try to

solve individual or community problems? [If yes,] Have you ever done any active work for political clubs? I mean, been a leader, helped organize meetings, or given time or money?"

Communal Activity. (1) "Have you ever worked with others in this community to try to solve some community problem?" (2) "Have you ever taken part in forming a new group or a new organization to try to solve some community problem?" (3) "Have you ever personally gone to see, or spoken to, or written to—some member of the local government or some other person of influence in the community about some need or problem? [If yes,] Was this need or problem primarily of concern to you, your friends and family, or was it an issue of wider concern?" (4) "What about some representative or governmental official outside of the local community—on the county, state, or national level? Have you ever contacted or written to such a person on some need or problem? [If yes,] Was this need or problem primarily of concern to you, your friends and family, or was it an issue of wider concern?" The final component of the communal activity item is a composite measure based on responses to questions concerned with *membership* in any of 15 possible voluntary associations (fraternal groups; service clubs; veterans groups; labor unions; sports groups; youth groups; school service groups; hobby and garden clubs; school fraternities and sororities; nationality groups; farm organizations; literary, art, discussion, or study groups; professional or academic societies; church affiliated groups; and any other groups), whether those groups worked to solve individual or community problems, and whether the respondent had been actively involved in the organization.

Particularized Contacting. See items 3 and 4 in the Communal Activity section

for question wording. Those who had contacted a local official or a nonlocal official on a personal matter are given high scores.

Creation of Participation Scales and Index

The participation measures were factor-analyzed using a principal components extraction and an oblique rotation, resulting in a four-factor solution. The factors identified correspond to Verba and Nie's (1972) voting, campaigning, communal involvement, and particularized contacting modes. We created scales for the items loading on each major mode by weighing each item by its factor loading, summing

and standardizing the measure and then multiplying it by 100. For each major mode this yields a scale with a mean of approximately 0 and a standard deviation of approximately 100. The summary participation index is based on a higher-order factor analysis using the four major mode scales, and the major mode scales are all positively intercorrelated. Each scale was weighed by its respective factor score and the sum of these was then standardized and multiplied by 100. Descriptive statistics for each measure are shown in Table 8, along with average interitem correlations and alpha coefficients. More complete details on scale and index creation can be obtained by writing to us.

Table 8. Descriptive Statistics for Participation Dependent Measures

Participation Measures	Minimum	Maximum	Mean	Standard Deviation	Average Correlation	Alpha
Voting (N = 1,699)	-181.40	89.71	.986	100.02	.64	.85
Campaigning (N = 1,746)	-75.42	435.85	-.005	102.10	.35	.73
Communal activity (N = 1,748)	-89.31	414.37	.016	105.69	.35	.72
Particularized contacting (N = 1,744)	-40.88	426.78	.000	105.38	.28	.46
Summary index (N = 1,745)	-145.45	380.91	.000	100.00	.24	.56

Notes

The data come from the 1987 General Social Survey, James A. Davis and Tom W. Smith, principal investigators. A module of new items was designed by a subcommittee of David Knoke (chair), Lawrence Bobo, and Thomas Guterbock, with the assistance of Duane F. Alwin. An earlier version of this paper was presented at the 1988 annual conference of the Association of Black Sociologists, Atlanta. This research was supported by NSF Grants SES-8615409 and BNS87-00864, by the Graduate School Research Committee of the University of Wisconsin, and was carried out while the first author was a fellow at the Center for Advanced Study in the Behavioral Sciences. We thank Emily E. Read for her helpful research assistance and Cedric Herring, Shanto Iyengar, Cora B. Marrett, John Petrocik, Stanley Presser, and David O. Sears for their comments on an earlier draft of this paper.

1. The PSUs scored as high-empowerment areas include Atlanta, Baltimore, Birmingham, Chicago, Dayton, Detroit, Los Angeles, Newark, Philadelphia, Richmond, and Washington. All other PSUs were scored as low-black-empowerment areas. We identified areas with black mayors using Joint Center for Political Studies (1986). Ideally, we would like to employ a more refined empowerment measure that could, for example, distinguish areas where blacks have held the mayor's office for several consecutive terms (e.g., Atlanta, Los Angeles, Detroit) from areas where this is a more recent, contested, and still-vulnerable accomplishment (e.g., Philadelphia, Chicago). We decided against such an approach because even with our relatively large black sample, we have too few black respondents in many PSUs to test for such differences efficiently and reliably.

2. Age and sex were included because our sample of blacks tends to be younger and include more women than the white sample. The adjustments

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were made by estimating an Ordinary Least Squares regression equation, pooled across race and weighted for the black oversample, of the form

$$y^* = b_0 - b_1(\text{educ}) - b_2(\text{occu}) - b_3(\text{faminc}) \\ - b_4(\text{age}) - b_5(\text{sex}),$$

where y^* = adjusted participation score, b_0 = intercept, $b_1 - b_5$ = OLS regression coefficients, educ = respondent's level of education, occu = respondent's (spouse's if respondent is not employed) occupational prestige (NORC Hodge-Siegel-Rossi scale), faminc = family income, age = respondent's age, and sex = dummy variable for sex (male = 0, female = 1).

3. Two possible complexities should be noted. First, we also found no pattern of significantly higher black participation at specific status levels; that is, we created socioeconomic status group quartiles based on a combination of education, occupational prestige, and family income. Black-white comparisons within the quartile groups using the adjusted participation scales revealed only one significant difference out of 16 possible comparisons (4 quartile groups by 4 participation scales), and this one favored whites over blacks. Second, political behaviors tend to be overreported (Abramson and Claggett 1986; Presser 1984), a tendency that may be exaggerated by our reliance on retrospective reports. But recent analyses suggest no substantial racial differential (Anderson and Silver 1986) and also suggest that validation data do not paint a substantially different picture of the determinants of participation than do self-reports in surveys (Katosh and Traugott 1981). Hence, we believe these data accurately gauge black-white differences in patterns of sociopolitical involvement and the relationship of participation to other factors.

4. In general, analyses of the major mode of participation scales support the summary index results shown in Table 3. Blacks in high-empowerment areas score as more active than comparable whites, net of socioeconomic status, age, and sex, for the voting, campaigning, communal activity, and particularized contacting scales. This difference is significant in the cases of voting (29.10, $p < .01$) and communal activity (24.92, $p < .05$), of borderline significance for campaign activity (22.68, $p < .08$), and in the right direction but insignificant for particularized contacting of officials (4.16). Among whites, those living in high-black-empowerment areas are significantly less likely than those in low-black-empowerment areas to report voting (-18.79, $p < .05$) and communal activity (-17.61, $p < .05$). Blacks in high-empowerment areas score as more active than blacks in low-empowerment areas for each major mode of participation scale, with this difference reaching significance for campaigning (28.50, $p < .01$) and voting (15.37, $p = .09$).

5. Ordinary Least Squares regression analyses show that level of empowerment significantly in-

creases scores on the political knowledge scale net of education, family income, occupation, age, sex, region (north vs. south), and size of place. (We included the two latter variables in order to assure that the empowerment variable was not simply capturing north-vs.-south or size-of-place differences.) Specifically, empowerment adds 1.6% to the total variance explained in political knowledge among blacks ($F = 8.33$, d.f. = 1/434, $p < .01$) and has the third largest effect in the equation (partial beta = .13, $p < .01$), following those for level of education (partial beta = .30, $p < .001$) and age (.25, $p < .001$).

6. The sort of political orientation typology we employ here, which distinguishes the politically engaged (high-trust, high-efficacy), the politically discontented (low-trust, high-efficacy), politically obedient (high-trust, low-efficacy), and politically alienated (low-trust, low-efficacy) is well established (Gutерbock and London 1983; Shingles 1981). Cells of the political orientation typology were created by dichotomizing the political trust and political efficacy scales at the black median (in order to assure adequate numbers of black respondents in each category) and then cross-classifying the trust and efficacy variables.

7. Analyses of the separate major mode of participation scales support the results reported in Table 5. Among blacks, there are significant differences by the orientation typology on the adjusted voting ($F = 4.09$, d.f. = 3/429, $p < .01$), campaigning ($F = 8.65$, d.f. = 3/443, $p < .001$), and communal activity scales ($F = 5.53$, d.f. = 3/443, $p < .01$), but not on the adjusted particularized contacting scale ($F = .16$, d.f. = 3/443, n.s.). Politically engaged blacks, net of socioeconomic status, age, and sex, are the most active orientation type among blacks in terms of voting, campaigning, and communal activity. Thus, even for very-high-initiative behaviors such as campaigning and communal activity, politically engaged blacks are more active than the politically discontented.

8. The effect of empowerment on black participation is most consistent in the electoral arena, that is, for voting and especially for campaigning activity. The empowerment variable has a small but significant zero-order correlation with black voting ($r = .12$) and campaigning ($r = .16$), a borderline correlation with communal activity ($r = .09$), and no relation to particularized contacting. Multiple regression analyses show that empowerment has a borderline effect on voting (partial beta = .07, $p = .10$), net of socioeconomic status, age, sex, region, and size of place. This borderline effect is eliminated on introducing the political orientation and knowledge variables. Empowerment has a highly significant effect on campaigning (partial beta = .17, $p < .001$), net of socioeconomic status, age, sex, region, and size of place. Furthermore, empowerment has a significant direct effect on campaigning (partial beta = .11, $p < .05$) even after controlling for political orientations and political knowledge. The multiple

regression analyses showed no net effect of empowerment on black levels of communal activity or particularized contacting of officials.

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STATE LOTTERY ADOPTIONS AS POLICY INNOVATIONS: AN EVENT HISTORY ANALYSIS

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Two types of explanations of state government innovation have been proposed: internal determinants models (which posit that the factors causing a state government to innovate are political, economic, and social characteristics of a state) and regional diffusion models (which point toward the role of policy adoptions by neighboring states in prompting a state to adopt). We show that the two are conceptually compatible, relying on Mohr's theory of organizational innovation. Then we develop and test a unified explanation of state lottery adoptions reflecting both internal and regional influences. The empirical results provide a great degree of support for Mohr's theory. For the empirical analysis, we rely on event history analysis, a form of pooled cross-sectional time series analysis, which we believe may be useful in a wide variety of subfields of political science. Event history analysis may be able to explain important forms of political behavior (by individuals, organizations, or governments) even if they occur only rarely.

Innovation by state governments has been a major topic of research by political scientists for two decades. Walker (1969) began the effort with his seminal study of the innovativeness of states across 88 programs. Gray's (1973) influential study of state innovation in the areas of civil rights, welfare, and education and Grupp and Richards's (1975) important study of policy diffusion in a wide variety of policy areas soon followed. Numerous other studies of state innovation have been published in the 1970s and 1980s, yielding insights into the determinants of innovativeness in a variety of policy areas. These include studies of innovation in juvenile corrections (Downs 1976), technology (Menzel and Feller 1977), consumer affairs (Sigelman and Smith 1980), energy (Regens 1980), tort

law (Canon and Baum 1981), judicial administration (Glick 1981), and human services (Sigelman, Roeder, and Sigelman 1981). But while expanding the scope of policy areas subject to innovation analysis, the research since 1975 has not led to major advances in our conceptualization of state innovation or our empirical approach to its investigation; the same basic approaches have simply been applied in new policy contexts.

A state government innovation has been defined as a "program or policy which is new to [the state] adopting it" (Walker 1969, 881), and the central research question about state innovation is, What causes a government to adopt a new program or policy? We claim that two fundamental answers have been offered. *Internal determinants* models posit that

AMERICAN POLITICAL SCIENCE REVIEW
VOLUME 84 NO. 2 JUNE 1990

the factors leading a state government to innovate are political, economic, and social characteristics internal to the state (Berry 1987). *Regional diffusion* models emphasize the influence of nearby states, assuming that states emulate their neighbors when confronted with policy problems.¹

A critical conceptual weakness in the state innovation literature is the segregation of these two types of explanations. Internal determinants models typically specify no role for regional influence (e.g., Downs 1976; Regens 1980), while regional diffusion models generally assume that internal state characteristics have no effect (e.g., Grupp and Richards 1975; Light 1978). Even when both models have been investigated within a single study, their analyses have been kept distinct, with internal determinants models cast as analyses of the determinants or correlates of policy innovation and regional diffusion models framed as analyses of policy emulation or diffusion (e.g., Canon and Baum 1981; Gray 1973; Walker 1969). The separate treatment of the two models in the literature indicates a failure to recognize that regional diffusion is not a separate topic from innovation but, instead, one possible explanation for innovation.

Furthermore, neither a pure regional diffusion model nor a pure internal determinants model is a plausible explanation of state innovation in isolation. It is unrealistic to assume that a state blindly emulates its neighbors' policies without its public officials being influenced by the political and economic environment of their own state. It is also implausible to presume that states are totally insulated from influence by neighboring states, given the context of federalism, active national associations of state officials, and media attention on state innovations. Furthermore, the regional diffusion and internal determinants models can be unified theoretically without doing violence to

either explanation. We show that both internal and regional influences on a state's likelihood of innovation can be predicted based on Mohr's (1969, 111) theory that the propensity to innovate is a function of "the motivation to innovate, the strength of obstacles against innovation, and the availability of resources for overcoming such obstacles."

We also offer a general empirical approach to studying innovation that allows for a test of a unified theory of state innovation reflecting both internal and regional effects and illustrate it with an analysis of state lottery adoptions. Our model of state lottery adoptions will be tested using pooled cross-sectional time series data, via *event history analysis*, a technique rarely used in political science but more common in other social and biological sciences.² We chose the lottery for illustration primarily because states have adopted it relatively recently (New Hampshire in 1964 was the first), thereby confining the period of analysis to years when data for the factors hypothesized to influence innovation are readily available. But also, no state has adopted a personal income tax or a general sales tax since 1976; in the 1980s, the lottery has become the most popular vehicle for adding a new means of "revenue enhancement" to state tax systems.³

Existing Approaches for Studying State Innovation

Testing Internal Determinants Explanations

Two very similar strategies have been used to test internal determinants models of state innovation. Both involve cross-sectional analysis in which the independent variables are state political, social, and economic characteristics. What differs is the dependent variable. In one set of studies, it is the year a policy was

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adopted (Canon and Baum 1981; Glick 1981; Gray 1973; Walker 1969);⁴ while in the other set, it is whether a state has adopted a policy by a specified date or not (Filer, Moak, and Uze 1988; Glick 1981; Regens 1980).

Both strategies have serious drawbacks. The weaknesses inherent in using *cross-sectional* analysis for making inferences about the nature of state policy-making processes (Gray 1976) are applicable to both. But apart from this general limitation, the choice of a year for measuring the independent variables is problematic when adoptions by states, as is typical, are spread over several decades. Since a cause must precede its effect, the only logical alternative is to use data for characteristics of states from the time a policy was first adopted. However, this can mean that later adoptions are being "explained" with characteristics of states several decades ago. Nor do these strategies allow us to assess the effects of state characteristics that vary substantially from year to year. For example, the hypothesis that "popular" policies tend to be adopted primarily in election years could not be tested with either of these cross-sectional strategies.

Testing Regional Diffusion Explanations

Three approaches have been tried for testing regional diffusion models. Walker (1969) uses factor analysis to discover clusters of states having similar orders of adoption for a variety of policies and then assesses whether states in the same cluster are in the same region of the country (see also Canon and Baum 1981). Of course, this approach is possible only when one examines several policies. Moreover, the failure of clusters to conform to regional contours could be due to a set of policies diffusing from different "starting states," thereby resulting in different orders of adoption for different policies, even if

each policy had indeed diffused regionally.

A second strategy has been to assess the relationship between adoptions by states and previous adoptions by their neighbors. Crain (1966) and Lutz (1986) both examine whether adoptions occur more frequently in jurisdictions with neighbors that have already adopted than in jurisdictions with no such neighbors. The key shortcoming of this approach is that one must assume that adoption of a policy by two neighboring states in close time sequence is evidence of regional influence, even though adoptions by neighbors at similar points in time may also result from the operation of similar internal factors in neighboring states.

A final strategy involves surveys of state officials (e.g., Freeman 1985; Grupp and Richards 1975; Light 1978; Menzel and Feller 1977). Officials are asked what states are leaders in a particular policy area or which officials in other states they consult for advice, and diffusion patterns are discerned from the responses. If we assume that officials' responses are accurate, this strategy can identify "true" regional influence as distinct from similarly timed adoptions by neighbors. But when state adoptions occur over many years, it is impractical to interview all officials immediately after their states adopt policies. And surveying officials in all states at the time a study is performed is inadequate, as the officials responsible for policy formulation in early-adopting states are likely to be gone or to have untrustworthy memories.

The Approach of This Study

We conceive of a program or policy adoption by a state as an event that may or may not occur in any given time period. Then the fundamental research question is, For any state, what determines the probability that the adoption event will occur during the time period?

Event history analysis (EHA) can be employed to answer this question.

In event history analysis, the goal is to explain a qualitative change (an "event") that occurs in the behavior of an individual at a particular point in time. (In our description of EHA, we will call the unit of analysis an "individual" even though in some research applications, the actual unit may be an organization, a state, a nation, or some other collective.) The data for analysis, called an event history, is a longitudinal record showing whether and when the event was experienced by a sample of individuals during a period of observation. In a *discrete time* model—the kind to be used in this study—the period of analysis is divided into a set of distinct units (e.g., years). But there are also *continuous time* EHA models that assume that the time of an event occurrence is measured exactly (Tuma and Hannan 1984). A critical concept in EHA is the *risk set*, which is the set of individuals in the sample that are "at risk" of event occurrence (i.e., have a chance of experiencing the event) at a particular time. When the event under analysis is one that an individual cannot repeat (e.g., death), the size of the risk set will decrease over time as individuals in the sample experience the event.⁵ Indeed, when observations are annual, the size of the risk set is decreased at the end of each year by the number who experienced the event that year.

The variable to be explained in discrete time EHA is called the *hazard rate* and is defined as the probability $P_{i,t}$ that an individual i will experience the event during a particular time period t , given that the individual is "at risk" at that time. The hazard rate is then presumed to be determined by a set of independent variables. Of course, the hazard rate, being a probability, is an *unobserved* variable. The *observed* dependent variable for estimating effects in EHA is a dummy variable that is scored one for each case when an

individual experiences the event, zero otherwise. The dichotomous nature of this variable makes probit or logit the preferable estimation technique.

Since most individual government programs can only be adopted once by a given jurisdiction, in applying event history analysis to the study of state policy innovation analysts will typically be dealing with nonrepeatable events. Thus, the *conceptual* dependent variable or hazard rate would be the probability of a state's adopting a policy during a particular period, given that it has not already adopted it in a previous period. While in theory the time unit under consideration can be quite short, data constraints (which typically will preclude more-than-annual observations for many independent variables) make a calendar *year* a sensible choice. It is reasonable to assume that no state is "at risk" of adopting a given program until after at least one state has given it serious consideration. And given the practical difficulty of determining precisely when the first serious consideration occurred, in most applications it would be appropriate to assume that no state is at risk of adopting prior to the year of adoption by the first state. The data set for analysis would then be pooled cross-sectional time series, in which the cases are "state-years." More precisely, the data would consist of one observation per state for each year the state is at risk of adopting, that is, for each year in which the state had not adopted prior to the beginning of the year. The observed dependent variable would be a dummy variable indicating whether a state adopts the policy in a given year.

Event history analysis has several critical advantages over the standard methodologies for innovation research reviewed above. First, unlike the extant methods, it is suitable for testing a unified theory of state innovation incorporating both internal determinants and regional influences. For such a test, some independent vari-

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ables in the EHA equation would be internal characteristics of states, while others would reflect the adoption behavior of nearby states. Furthermore, including both regional and internal influences in the same model guards against mistaking a spurious relationship between states' years of adoptions and those of their neighbors (actually due to the operation of similar internal characteristics in neighboring states) as evidence of regional diffusion. Presumably, if the relationship were spurious, the estimated effects of terms representing the behavior of nearby states would diminish to near zero in the EHA equation, as these regional effects would be appropriately "controlled" for the impacts of internal characteristics.

Moreover, unlike the traditional cross-sectional methods for testing internal determinants models, EHA can assess the effects on the probability of adoption of characteristics of states that vary substantially from year to year, as annual longitudinal variation is incorporated in the data set. Also, and again unlike cross-sectional methods, the "pooled" nature of our data allows the dependent variable to be affected by independent variables with the right time property. It is not necessary to assume that a state adopting a program recently is affected by what its characteristics were when the first state adopted the program perhaps decades ago.

Finally, the use of EHA to study state innovation should dramatically increase the substantive relevance of research findings in the literature. The traditional approaches are capable of predicting only (1) whether a particular type of state should have adopted a policy prior to a specified date or not, or (2) the timing of a state's adoption relative to adoptions by other states. But EHA is equipped to yield more interesting conclusions. In particular, EHA can predict the probability that a particular type of state will adopt a policy during a particular year.

The extremely limited variance typical-

ly present in the adopt-versus-not-adopt-in-a-given-year observed dependent variable may have deterred others who considered using a pooled cross-sectional time series approach for studying innovation. Since most programs can be adopted only once by each state and the years of adoption by different states can be spread over several decades, in a pooled data set consisting of cases "at risk" of adoption, the percentage of cases scored *adopt* can be less than 5%. While this may appear to be an almost insurmountable obstacle to fruitful empirical research the very low variance for the dependent variable in our lottery adoption analysis, as we will see, does not thwart our ability to conduct meaningful empirical research. Even though lottery adoptions are quite unusual events, we can study empirically the factors that lead states to adopt them.

A Unified Theory of State Innovation: Incorporating Internal and Regional Influences

Mohr's (1969) analysis of organizational innovation provides a foundation for building a theory that integrates the internal determinants and regional diffusion models of state innovation. Mohr (1969, 114) argues persuasively that the probability of innovation is inversely related to the strength of obstacles to innovation and directly related to (1) the motivation to innovate, and (2) the availability of resources for overcoming obstacles. He further hypothesizes that the motivation to innovate interacts with both the strength of obstacles and the amount of resources available in influencing the chances of adoption (p. 123). In particular, "when the obstacles are relatively great, and the resources small," even a high level of motivation should not produce innovation. But as obstacles diminish and resources rise, the impact of motivation on the probability of innovation

should increase. Conversely, when there is little motivation to innovate, the level of resources and the strength of obstacles are unlikely to have much effect on the probability of innovation, as without sufficient motivation the probability of innovation should be uniformly low. But as motivation to innovate increases, the influence of both resource availability and the strength of obstacles should grow.

It is clear that numerous internal determinants of innovation in a state can be conceived as reflecting the motivation of politicians to innovate, the obstacles they face, or the resources available to overcome these obstacles. The issue is whether regional influences are consistent with Mohr's theory, as we maintain. Elazar (1972) claims that state policy makers tend to view nearby states as "experimental laboratories" for policies. The consequences of adopting a new program can be very difficult to predict; information about effects of the program in a similar state can help overcome the uncertainty. Thus, policy adoptions by nearby states provide a critical resource (information) for overcoming an obstacle (uncertainty) to innovation. Furthermore, with a policy unpopular with the electorate, it should be easier for politicians to justify its adoption to voters if it has first been adopted by nearby states. Thus, again, the presence of previously adopting nearby states becomes a resource useful for overcoming an obstacle to innovation (in this case, negative public opinion). With a policy that is generally popular with voters, the existence of previously adopting nearby states should intensify internal political pressures to adopt, as voters see a popular policy in place in nearby states and want it in their state as well. Consequently, as a greater number of nearby states adopt a popular policy, the motivation of a state's politicians to adopt is heightened. Hence, regional influences on state innovation are fully predictable based on Mohr's theory.

This insight suggests that a unified theory of the causes of state innovation, relying on both internal and regional influences, can be developed. Indeed, the recognition that previously adopting nearby states can be a resource for overcoming obstacles to innovation—combined with Mohr's hypothesis that "level of resources" interacts with "motivation to innovate" in influencing the probability of innovation—suggests that the strength of regional influences on a state's probability of innovation should vary depending on the internal environment in a state. If so, both *pure* internal determinants and regional diffusion models would not only be incomplete explanations of state innovation, but the failure to incorporate either of these sources of influence (internal or regional) in a theory of state innovation may actually prevent the discovery of empirical support for the other. In the next section, we use Mohr's theory to develop an explanation for the adoption of state lotteries (1) reflecting both internal and regional influences on the probability of innovation and (2) specifying how the two should interact.

An Explanation of State Lottery Adoptions

Our model assumes that the probability that a state without a lottery will adopt one in a given year is determined by both the state's internal characteristics and the previous pattern of lottery adoptions by nearby states. Research about state *tax* innovation (principally Hansen 1983) is useful in theorizing about the determinants of lottery adoptions, as factors that create the need for increased revenues can prompt the adoption of either a lottery or a new sales or income tax. But hypotheses about the determinants of (sales or income) tax adoptions cannot be translated to a study of the lottery without careful consideration of their applicability. While

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lotteries and taxes are both revenue collection mechanisms, they have fundamental differences that affect the politics of their adoption. First, while citizen payments of sales and income taxes are mandatory, participation in lotteries is strictly voluntary. And this difference is likely responsible for a second: in contrast to voter antipathy to new sales and income taxes, new state lotteries tend to be quite popular with state electorates (Mikesell and Zorn 1986).

Hypotheses Concerning the Motivation To Innovate

Both economic and political conditions can be expected to affect the motivation of state political officials to adopt a lottery. The most important economic determinant of motivation should be the short-term fiscal health of a state's government. Hansen (1983, 150) finds that states do not adopt sales and income taxes during prosperous times. But during periods of financial hardship, both tax adoptions and tax increases are more likely (see also Mikesell 1978). Hansen believes that this is true because an economic crisis reduces the political risks to public officials of adopting a new (mandatory) tax. Of course, sales and income taxes are uniformly unpopular with state electorates. Even though there are not the same political risks when adopting the generally popular lottery, budget shortfalls should still increase the motivation of politicians to seek new revenues by adopting a lottery.

HYPOTHESIS 1. *The worse the fiscal health of a state's government—that is, the greater its expenditures relative to its revenues—the more likely it is to adopt a lottery.*

The most critical aspect of the political environment determining the motivation of public officials to adopt a lottery should be the proximity of state elections.

Tufte (1978; see also Kiewiet and McCubbins 1985) maintains that politicians have incentives to adopt new policies at times within the election cycle that are most advantageous politically. Since increases in sales and income taxes face substantial popular opposition, new mandatory taxes should be likeliest to be enacted in the year following elections, thereby giving the public the maximum amount of time to forget the government's unpopular action before the next election. But the general popularity of the lottery among state electorates makes it likely that elected officials perceive that adopting a lottery in an election year would enhance their chances for reelection. To simplify the analysis, we focus on elections for governor, ignoring the effects of legislative elections; this choice is supported by Bingham, Hawkins and Hebert (1978), who contend that governors have been more active than legislatures in defining state taxation agendas.

HYPOTHESIS 2. *For all states a lottery is most likely to be adopted in an election year. In states with more than two years between gubernatorial elections, adoption is least likely in the year immediately following an election.*

Moreover, we expect the natures of the political and economic environments to interact in influencing the probability of a lottery adoption. When a state's treasury is fiscally healthy, public officials are unlikely to adopt a lottery even if it is an election year. But if a state is in poor fiscal health, whether a lottery is adopted or not should be more strongly influenced by the proximity of elections. During an election year, a lottery may seem like the ideal solution to politicians, but in a year following an election, politicians may prefer to rely on relatively unpopular actions (like cutting spending or increasing mandatory taxes), thereby reserving the lottery option for a situation in which a

fiscal crisis occurs during an election year. According to this view, both poor fiscal health and being in an election year are necessary conditions for adopting a lottery.

HYPOTHESIS 3. *The poorer the fiscal health of a state's government, the stronger the effect of elections on the probability that the state will adopt a lottery. Similarly, the fiscal health of a state should have a stronger impact on the probability of adoption in an election year than in a year after an election.*⁶

Hypotheses Concerning the Obstacles to Innovation

Two potential obstacles to adopting a lottery are (1) organized constituencies opposed to a lottery and (2) a state population with insufficient financial resources to support a lottery adequately. In particular, a low level of personal income in a state can be viewed as an obstacle to a successful lottery. While state lotteries are generally regressive in their incidence (Suits 1977), lottery participation rates are still highest among middle- and upper-income levels (Mikesell and Zorn 1986, 315). Therefore, the lower the average income in a state, the greater should be the concern by public officials that a lottery will be unsuccessful in raising revenues.

HYPOTHESIS 4. *The lower the level of personal income in a state, the lower the probability that the state will adopt a lottery.*⁷

State officials concerned about reelection must also be sensitive to any strongly held beliefs against the lottery among the electorate. The overall popularity of the lottery masks intense resistance to the lottery on moral grounds by religious fundamentalists. Studies have found that membership in fundamentalist religions is a determinant of (1) the restrictiveness of

liquor and gambling regulations in the states (Fairbanks 1977) and (2) outcomes of referenda on liquor, gambling, and Sunday business issues (Meier and Morgan 1980). It also makes sense that the larger the percentage of a state's population that adheres to fundamentalist religions (which view gambling as sinful) the less likely the state is to adopt a lottery, as a large fundamentalist population increases the potential political costs to government officials of supporting a lottery.

HYPOTHESIS 5. *The greater the proportion of a state's population adhering to fundamentalist religions, the lower the probability that the state will adopt a lottery.*

But as we have seen, Mohr (1969) asserts that the strength of obstacles to innovation interacts with the motivation to innovate in influencing the probability of adoption: the greater the level of motivation, the greater the effect of "strength of obstacles" on "likelihood of innovation." In our analysis fiscal health and election proximity are presumed to be the principal determinants of the motivation to innovate, and fundamentalist opposition and low personal income are the obstacles.

HYPOTHESIS 6. *The effect of religious fundamentalism as an obstacle to lottery adoption is greater in an election year and when a state's fiscal health is poor than in a year after an election and when fiscal health is stronger.*

HYPOTHESIS 7. *The effect of low personal income as an obstacle to lottery adoption is greater in an election year and when a state's fiscal health is poor than in a year after an election and when fiscal health is stronger.*

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Hypotheses Concerning Resources for Overcoming Obstacles

Two kinds of political resources are expected to help state officials overcome obstacles to adopting a lottery: unified party control of government and previous adoptions by nearby states. Susan Hansen (1983, 153–54) hypothesizes that states in which the governorship and both houses of the legislature are controlled by the same political party are more likely to adopt a tax than states in which governmental institutions are under divided party control, regardless of which party is in power. This is because a unified government can better avoid the “roadblocks” resulting from the need for compromise between two parties. The need for a unified government may be greater when considering the adoption of a controversial mandatory tax than when contemplating adopting a more popular lottery. But Hansen’s logic seems applicable to the case of lottery adoptions as well, as unified governments should be more capable than divided governments of achieving the necessary consensus on the specific nature of a lottery (e.g., whether the revenues generated are to go into a state’s “general fund” or be earmarked for a particular public service).⁸

HYPOTHESIS 8. *When a single political party controls the governorship and both houses of the legislature, the probability that the state will adopt a lottery is greater than when the government is under divided party control.*

We argue above that previous adoptions by nearby states can also provide an important resource for overcoming obstacles to innovation, as such adoptions yield important information about a policy’s effects. The logic supporting this proposition is applicable in the case of the lottery, thereby suggesting that the probability of a lottery adoption increases as a

greater number of nearby states adopt it. But specifying this hypothesis requires us to be more precise about the meaning of a “nearby” state. The literature suggests several possibilities.

First, the states could be divided into predesignated regions with the hypothesis that a state’s probability of adopting a lottery increases as the number of states in its region that have previously adopted it gets larger. But this approach has significant weaknesses. The variety of different regional demarcations in the literature, with different numbers of regional clusters and different groupings of states within these clusters (see esp. Sharkansky 1970) illustrates the difficulty of justifying any particular demarcation. While one might introduce a theoretical argument in support of one demarcation or another (e.g., Elazar 1972), the choice of how to define regional clusters remains largely arbitrary. Furthermore, whenever predesignated regions with fixed boundaries are defined, some states that border each other necessarily wind up in different regions. So in testing a regional influence hypothesis, the impact of some neighboring states would inevitably be ignored.

A second conception of regional influence would involve both predesignated regions and predesignated leader states within those regions. We would hypothesize that a state’s probability of adopting a lottery increases after one or more states with a reputation as a leader within its region adopt it. This definition is consistent with research that has found that there are states to which the other states in a region look most frequently for innovative ideas (Grupp and Richards 1975; Menzel and Feller 1977). This conception of regional diffusion is most attractive when there are reliable data about which states are perceived by public officials to be regional leaders in a policy area. Unfortunately, we have no such data for lotteries.

The conception of regional influence used in this study defines “nearby” states

as immediate neighbors (i.e., states that share a boundary). The advantage of this definition is that we are not required to assign states to predesignated regions arbitrarily. Instead, all of a state's neighbors that have previously adopted a lottery are assumed to be influential in promoting innovation. Indeed, a conception of regional influence that focuses on *immediate* neighbors seems especially appropriate in the case of the lottery. When a state adopts a lottery and a neighboring state does not have one, people living near the border in the neighboring state can cross the border to purchase tickets. This places pressure on state officials to adopt a lottery to try to keep a state's own "tax base" from being taxed by a neighbor.

HYPOTHESIS 9. *The probability that a state will adopt a lottery is positively related to the number of states that border it that have already adopted.*

Mohr (1969) also argues that the availability of resources for overcoming obstacles to innovation interacts with the motivation to innovate in determining the probability of adoption. When the level of motivation to innovate is high, the effect of "resource availability" on "likelihood of innovation" is stronger than when motivation to innovate is low.

HYPOTHESIS 10. *The effect of unified political party control in overcoming obstacles to lottery adoption is greater in an election year and when a state's fiscal health is poor than in a year after an election and when fiscal health is stronger.*

HYPOTHESIS 11. *The effect of previously adopting neighboring states in overcoming obstacles to lottery adoption is greater in an election year and when a state's fiscal health is poor than in a year after an election and when fiscal health is stronger.*

An Event History Analysis Model of Lottery Adoption

These 11 hypotheses combine to suggest the following EHA model:

$$\begin{aligned} \text{ADOPT}_{i,t} = \Phi & (b_1 \text{FISCAL}_{i,t-1} \\ & + b_2 \text{PARTY}_{i,t} + b_3 \text{ELECT1}_{i,t} \\ & + b_4 \text{ELECT2}_{i,t} + b_5 \text{INCOME}_{i,t-1} \\ & + b_6 \text{RELIGION}_{i,t-1} \\ & + b_7 \text{NEIGHBORS}_{i,t}) \end{aligned} \quad (1)$$

where the conceptual dependent variable or hazard rate $\text{ADOPT}_{i,t}$ is the probability that state i will adopt a lottery in year t , given that the state has not adopted a lottery prior to year t , and Φ denotes the cumulative normal distribution function. Thus, equation 1 takes the form of a probit model.⁹ $\text{ADOPT}_{i,t}$ is measured with a dummy variable equaling one if state i adopts a lottery in year t , zero otherwise.

In the equation, $\text{FISCAL}_{i,t-1}$ denotes the fiscal health of a state's government in the previous year. To control for size differences across states, fiscal health is measured by the ratio of total-state-revenue-minus-total-state-spending to total spending. Several independent variables—including FISCAL —are measured in the *previous* year, since legislative sessions typically begin in January so that legislators must often make policy based on the prior year's fiscal and economic data. Moreover, if revenue and expenditure data from the same year that a lottery adoption occurred were used to measure FISCAL , the adoption might generate revenues that would go into the calculation of FISCAL . To make certain that FISCAL may cause ADOPT , but ADOPT may not affect FISCAL , FISCAL must be "lagged" behind ADOPT . (For more specific descriptions of the indicators for the independent variables in equation 1, see the Appendix.)

$\text{INCOME}_{i,t-1}$ represents personal income, as measured by real per capita in-

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come, in state i in the previous year. RELIGION is the proportion of a state's population adhering to fundamentalist religions. PARTY is the degree to which a single political party controls the institutions of state government; it is operationalized with a dichotomous variable distinguishing situations in which the governor and the two legislative houses are controlled by the same party from situations in which there is split control (see Hansen 1983). NEIGHBORS $_i$, the term reflecting regional influence, denotes the number of previously adopting neighboring states, that is, the number of states sharing a border with state i that had adopted a lottery prior to year t . Equation 1 was also estimated using an alternative measure of NEIGHBORS, the *percentage* of states sharing a border that had previously adopted a lottery. The resulting coefficient estimates are quite similar to those based on number of previous adopters.

Finally, two dummy variables are included to specify the election cycle hypothesis: ELECT1, which equals one in the year of a gubernatorial election, zero otherwise; and ELECT2, which equals one if it is neither the year of an election nor the year after an election, zero otherwise. Our hypotheses predict that the coefficients for FISCAL and RELIGION will be negative, and that those for PARTY, INCOME, NEIGHBORS, ELECT1 and ELECT2 will be positive. Furthermore, we predict that the coefficient for ELECT1 should be greater than that for ELECT2. This would mean that in states with four-year gubernatorial terms—the modal length among states—the probability of a lottery adoption is highest in an election year, lowest in a year immediately following an election, and in between these two values in other years.

Empirical Analysis of the Model of State Lottery Adoption

The first task in testing an EHA model is defining the risk set. Because our model

specifies effects by neighboring states, the sample is confined to the forty-eight continental U.S. states. Since the lottery was not adopted by any state until New Hampshire did so in 1964, we confine the analysis to observations from 1964 and later. Once a state adopts a lottery, it is no longer at risk of adopting. But states not adopting the lottery prior to the last year represented in our data set, 1986, are presumed to remain at risk of adopting through 1986. So the data set includes a varying number of observations for the states. The time series for the dependent variable for New Hampshire consists of a single 1 in 1964. The dependent variable time series for each of the remaining adopting states consists of a series of 0s beginning in 1964 and ending in the year before the state adopted the lottery, followed by a single 1 in the year of adoption. Finally, for a state not adopting the lottery by 1986, the time series for the dependent variable has no variation; it is a series of 0s starting in 1965 and ending in 1986.¹⁰

Equation 1 is estimated with pooled cross-sectional time series probit, and the resulting maximum likelihood estimates (MLEs) are presented in Table 1, column 1. Overall, the support for the model is exceedingly strong. Despite the fact that lottery adoptions are quite rare, with only 3% of the observations in our sample scored as adoptions, nearly all hypotheses receive support. These include Hypotheses 1 and 2 about "motivation" factors. The negative coefficient for FISCAL confirms that as expected, a decline in a state's fiscal health increases the probability of its adopting a lottery. But the political climate also matters. The fact that the coefficients for ELECT1 and ELECT2 are both positive, while the former is larger, implies that lottery adoptions are most likely in election years and least likely in years immediately after elections. In states with a gubernatorial election every four years the probability of a lottery adoption is highest in an election year,

**Table 1. Probit Maximum Likelihood Estimates
for Event History Analysis Model of Lottery Adoption**

Independent Variables	With PARTY (1)		Without PARTY (2)	
	Maximum Likelihood Estimate	t-ratio	Maximum Likelihood Estimate	t-ratio
ELECT1 _{i,t}	.82*	2.34	.79*	2.31
ELECT2 _{i,t}	.59*	1.71	.56*	1.68
INCOME _{i,t-1}	.023***	3.34	.23***	3.33
FISCAL _{i,t-1}	-1.69	-1.30	-1.82	-1.44
PARTY _{i,t}	-.40*	-1.83	—	—
RELIGION _{i,t-1}	-.034*	-2.11	-.035*	-2.23
NEIGHBORS _{i,t}	.27***	2.86	.25**	2.78
Intercept	-4.51***	-5.46	-4.62***	-5.64
Number of cases ^a	857		901	
Percentage of cases scored as adoption	.031		.029	
-2(Log-likelihood ratio) ^b	60.73***		57.37***	
Estimated R squared ^c	.48		.44	

Note: All significance tests are one-tailed except for those of intercepts, which are two-tailed.

^aThe sample for estimation in column 1 excludes Minnesota and Nebraska in years in which they had non-partisan legislatures, as PARTY is not defined for such cases.

^bMinus 2 multiplied by the log-likelihood ratio is distributed as chi-square (with seven degrees of freedom in column 1, and six degrees of freedom in column 2).

^cR-squared as reported by the McKelvey-Zavonia probit package; see Aldrich and Nelson (1984, 57-59) for a description of this measure.

* $p < .05$.

** $p < .01$.

*** $p < .001$.

decreases in the following year, and then increases again for the next two years as an election gets closer but not to as high a level as during an election year. This finding suggests that politicians do seek to adopt popular policies during election years, when the accompanying electoral rewards should be at their maximum.

Hypotheses 4 and 5 concerning obstacles to innovation are also confirmed. The positive coefficient estimate for INCOME (significant at the .001 level) is consistent with the proposition that politicians perceive low state personal income as an obstacle to a successful lottery. As predicted, the lower the level of per capita income, the lower the probability of a lottery adoption. Moreover, state officials seem to be influenced by opposition to the lottery on religious grounds. The likeli-

hood of a lottery adoption decreases as the share of a state's population adhering to fundamentalist religions (RELIGION) increases.

There is mixed empirical evidence about the hypotheses concerning availability of resources. The regional influence proposition (Hypothesis 9) receives strong support. The positive and statistically significant coefficient estimate for NEIGHBORS suggests that the probability that a state will adopt a lottery increases as the number of its neighbors that have previously adopted it grows, even when the effects of "internal" characteristics have been controlled.

But Hypothesis 8—which predicts that governments controlled by a single political party are more likely to adopt a lottery than those under split control—fails

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to receive support. Indeed, the opposite relationship seems to hold. Governments under split control are more likely to adopt than those that are unified. As we have noted, Hansen (1983) postulates that unified governments are more likely to adopt sales and income taxes than divided governments. We extended that logic to the case of the lottery. But it may be that unified governments, when motivated to increase state revenues, seek to capitalize on their monopoly control over the institutions of government to achieve a *substantial* tax increase by either adopting a new sales or income tax or raising existing tax rates. If, in contrast, divided governments lack the political resources to increase unpopular mandatory taxes and must instead settle for a less controversial lottery adoption, this might account for our finding that divided governments are more likely to adopt a lottery than unified governments.

A great advantage of event history analysis for state innovation research is that the coefficient estimates it generates can be used to calculate predicted probabilities that a state with specified characteristics will adopt a policy in any given year. These predicted probabilities can offer analysts powerful substantive conclusions—not available from traditional cross-sectional approaches to innovation research—about the magnitudes of effects of the factors determining adoption likelihood. Moreover, an analysis of such predicted probabilities allows researchers to assess the nature of interactions among the determinants of adoption probability.¹¹

Table 2 presents predicted probabilities of a lottery adoption (derived from probit MLEs) for hypothetical states with differing characteristics. To enhance the interpretability of the predicted probabilities, we deleted the unified government variable (PARTY) from the EHA model and calculated the probabilities associated with this revised model. (The MLEs for the equation excluding PARTY are in

Table 1, column 2.) We deleted party because (1) its MLE is fairly strong but in a direction contrary to that hypothesized and (2) since it is dichotomous, it would have to be fixed at one of its two extremes (rather than a “central” value) when calculating predicted probabilities, thereby magnifying its effect.

Each grouping (of three or seven lines) in Table 2 shows the change in the predicted probability of a lottery adoption that results when one independent variable is changed from one extreme to another while the remaining independent variables are held constant at specified values. (For ease of viewing, the values of variables that are “changing” are denoted in the table in italics, while values of variables being held constant are in roman type.) These predicted probabilities permit us to assess the hypotheses predicting interaction among the factors influencing the probability of a lottery adoption.¹²

Hypothesis 3 receives support from the first section of Table 2. The predicted probabilities show that the effect of elections on the probability of a lottery adoption does depend on the fiscal health of the state. The top half of the first section shows that the impact of elections is very small when fiscal health is exceptionally good and the other determinants of adoption probability are at “central” or “moderate” values. In particular, in a hypothetical state with (1) no lottery, (2) very good fiscal health, (3) a gubernatorial election every four years, (4) per capita income and percentage fundamentalist population at their average values across cases in the sample, and (5) two neighboring states that have previously adopted a lottery, the expected probability of a lottery adoption decreases only slightly from .028 in an election year to .003 the year after (for a probability difference of .025).¹³ But as fiscal health deteriorates, the effect of elections intensifies. As seen in the bottom half of the first section, a hypothetical state that is in very poor fiscal health but

**Table 2. Predicted Probabilities of Lottery Adoption for Hypothetical States
with Gubernatorial Elections Every Four Years***

Hypothetical Conditions	FISCAL	Year in Election Cycle	INCOME	NEIGHBORS	RELIGION	Probability of Adoption of a Lottery in a Year
Amount elections vary with fiscal health						
Excellent fiscal health	.01	<i>election year</i>	89.5	2	17.6	.028
	.01	<i>postelection year</i>	89.5	2	17.6	.003
	.01	<i>other two years</i>	89.5	2	17.6	.016
Poor fiscal health	-.20	<i>election year</i>	89.5	2	17.6	.063
	-.20	<i>postelection year</i>	89.5	2	17.6	.010
	-.20	<i>other two years</i>	89.5	2	17.6	.039
Amount fiscal health varies with the proximity of elections						
Postelection year	-.20	postelection year	89.5	2	17.6	.010
	-.09	postelection year	89.5	2	17.6	.006
	.01	postelection year	89.5	2	17.6	.003
Election year	-.20	election year	89.5	2	17.6	.063
	-.09	election year	89.5	2	17.6	.042
	.01	election year	89.5	2	17.6	.027
Amount religious fundamentalism varies with the motivation to innovate						
Postelection year, moderate fiscal health	-.09	postelection year	89.5	2	.7	.026
	-.09	postelection year	89.5	2	17.6	.006
	-.09	postelection year	89.5	2	34.4	.001
Election year, moderate fiscal health	-.09	election year	89.5	2	.7	.125
	-.09	election year	89.5	2	17.6	.042
	-.09	election year	89.5	2	34.4	.010
Election year, poor fiscal health	-.20	election year	89.5		.7	.171
	-.20	election year	89.5	2	17.6	.063
	-.20	election year	89.5	2	34.4	.017
Amount personal income varies with the motivation to innovate						
Postelection year, moderate fiscal health	-.09	postelection year	41.3	2	17.6	.000
	-.09	postelection year	89.5	2	17.6	.006
	-.09	postelection year	135.9	2	17.6	.072
Election year, moderate fiscal health	-.09	election year	41.3	2	17.6	.002
	-.09	election year	89.5	2	17.6	.042
	-.09	election year	135.9	2	17.6	.249
Election year, poor fiscal health	-.20	election year	41.3	2	17.6	.004
	-.20	election year	89.5	2	17.6	.063
	-.20	election year	135.9	2	17.6	.317

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Table 2 (continued)

Hypothetical Conditions	FISCAL	Year in Election Cycle	INCOME	NEIGHBORS	RELIGION	Probability of Adoption of a Lottery in a Year
Amount regional influence varies with the motivation to innovate						
Postelection year, moderate fiscal health	-.09	postelection year	89.5	0	17.6	.001
	-.09	postelection year	89.5	1	17.6	.003
	-.09	postelection year	89.5	2	17.6	.006
	-.09	postelection year	89.5	3	17.6	.012
	-.09	postelection year	89.5	4	17.6	.022
	-.09	postelection year	89.5	5	17.6	.039
Election year, moderate fiscal health	-.09	postelection year	89.5	6	17.6	.065
	-.09	election year	89.5	0	17.6	.013
	-.09	election year	89.5	1	17.6	.024
	-.09	election year	89.5	2	17.6	.042
	-.09	election year	89.5	3	17.6	.069
	-.09	election year	89.5	4	17.6	.110
Election year, poor fiscal health	-.09	election year	89.5	5	17.6	.164
	-.90	election year	89.5	6	17.6	.234
	-.20	election year	89.5	0	17.6	.021
	-.20	election year	89.5	1	17.6	.037
	-.20	election year	89.5	2	17.6	.063
	-.20	election year	89.5	3	17.6	.100
	-.20	election year	89.5	4	17.6	.152
	-.20	election year	89.5	5	17.6	.218
	-.20	election year	89.5	6	17.6	.300

Note: FISCAL = -.20, -.09, or .01 indicates state government fiscal health at the tenth percentile, mean, or ninetieth percentile, respectively, of the actual distribution of fiscal health scores among the cases in the sample. INCOME = 41.3, 89.5, or 135.9 indicates real per capita income at its lowest actual level, its mean level, or its highest level, respectively, among the cases in the sample. RELIGION = .7, 17.6, or 34.4 indicates a percentage of state population adhering to fundamentalist religions at its lowest actual level, its mean level, or its highest level (except for one outlier, Utah, at 75.9), respectively, among states in 1971. Only five states are bordered by seven or eight immediate neighbors; the predicted probability of adoption of a lottery for a state bordered by seven or eight previously adopting states is not calculated because for all but these five states, the existence of more than six previously adopting states is physically impossible.

*Based on Event History Analysis Probit MLEs in Table 1, column 2.

has "central" values on other variables, has a .053 (= .063 - .010) greater chance of adopting a lottery during an election year than in the year immediately after.

The interaction between fiscal health and election proximity is also evidenced in the second section of Table 2, which shows clearly that the effect of fiscal health is dependent on the proximity of elections. In the year after an election—and at "central values" for other indepen-

dent variables—fiscal health is virtually unrelated to the probability of a tax adoption (see the top half of the second section). But in an election year, fiscal health has an effect, albeit small, on the chance of an adoption. The predicted likelihood of a lottery adoption increases by .036 (from .027 to .063) when a state government's fiscal health deteriorates from very good to very poor.

Moreover, as we hypothesized, the

fiscal health of a state's government and the proximity of elections appear to be important contextual motivation factors that determine how obstacles to innovation and resource availability impinge on the probability that a state will adopt a lottery. With respect to the interaction between motivation to innovate and obstacles to innovation, Hypotheses 6 and 7 both receive striking support. The effects of religious fundamentalism and low personal income as obstacles to lottery adoption increase as (1) a state's fiscal health deteriorates and (2) it gets closer to an election. In particular, when it is the year after a gubernatorial election and a state is in moderate fiscal health (and has central values for the other variables as well), the strength of religious fundamentalism has only a very slight impact on the probability of a lottery adoption (see the top third of the third section)¹⁴ and personal income has a relatively small effect (see the top third of the fourth section). But when the same state in moderate fiscal health is in an election year, membership in fundamentalist religions has a stronger negative relationship to the chance of adoption (see the middle third of the third section), and per capita income has a stronger positive relationship (see the middle third of the fourth section). Finally, if it is an election year and a state is in very poor fiscal health, religious fundamentalism and personal income exert even stronger influences on the probability of a lottery adoption (see the bottom third of the third and fourth sections). For example, even a state in poor fiscal health during an election year is predicted to have virtually no chance (.004) of adopting a lottery if personal income is extremely low; but when income is very high, the probability increases to .317.

Finally, there is evidence that regional influence on the probability of a lottery adoption varies depending on the level of motivation to innovate, as predicted by Hypothesis 11.¹⁵ When it is a year follow-

ing an election and a state is in moderate fiscal health, the number of its neighbors having lotteries has only a slight effect on its probability of adoption; a state in the year after an election and moderate fiscal health with as many as four neighbors having lotteries has only a .021 ($= .022 - .001$) greater predicted probability of adoption than a state with no such neighbors (see the top third of the fifth section). But if fiscal health remains moderate and it is an election year, the effect of previously adopting neighbors on the likelihood of adoption is stronger (see the middle third of the fifth section). Finally, the bottom third of the fifth section shows that the effect of neighboring states is still stronger when a state is both in an election year and in poor fiscal health.

Conclusion

Our empirical analysis offers a great deal of support for our unified model of state lottery adoptions. There is evidence for both the internal determinants and regional diffusion models of state innovation, as both (1) internal political and economic characteristics of a state and (2) the number of previously adopting neighboring states are found to influence the probability of a lottery adoption. Also, these two dominant explanations of state innovation are in no sense inconsistent. We have seen that expectations of both internal and regional influences can be derived from Mohr's theory of innovation. And our study of lottery adoptions confirms all essential elements of Mohr's theory. The probability of state innovation is directly related to the motivation to innovate, inversely related to the strength of obstacles to innovation, and directly related to the availability of resources for overcoming these obstacles. Moreover, Mohr's assertion that these three critical determinants interact in their influence on the probability of innovation receives

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consistent support. One such interaction in the context of lottery adoptions is between the influences of (1) neighboring states, and (2) "internal" factors reflecting the motivation to innovate (i.e., fiscal health and election proximity). Neighboring states are found to have a stronger impact on the likelihood of a lottery adoption when the internal characteristics of a state are themselves favorable for innovation (e.g., poor fiscal health and an election year). This reinforces our claim that regional diffusion and internal determinants explanations of state innovation should not be analyzed in isolation; instead, unified models are needed.

Our study also has other implications for future political science research. First, our findings suggest that scholars of state innovation should not be deterred by the fact that they are almost always attempting to explain rare events. State lottery adoptions are very unusual events; only 3% of the cases in our sample are scored *adoption*; fully 97% of the cases are state-years in which no lottery was adopted. But despite this fact, we can explain quite well when and why lottery adoptions occur. While we cannot be certain that similar studies of innovation in other policy areas would be as successful, we believe our findings are sufficiently promising to encourage students of state innovation to undertake event history analyses of other types of policy adoptions.

With event history analysis, scholars can subject theories of state government innovation to a powerful test by assessing whether these theories can predict the probability that a particular type of state will adopt a particular policy in a particular year. We believe that such analyses can yield conclusions about the factors encouraging innovation with considerably greater substantive relevance than studies using methodologies dominant in the literature to date. Moreover, event history analysis has proven successful for testing (and supporting) a theory of inno-

vation assuming that both a state's internal characteristics and nearby states influence the probability of a policy adoption. The methodologies used in previous innovation research have precluded empirical evidence for this expectation.

Finally, we hope that our success in using event history analysis to explain state adoptions of lotteries will encourage scholars in other subfields of political science to consider EHA's potential as an empirical tool. With it one might study a wide range of political events and in doing so take advantage of both temporal and cross-sectional variation in political behavior. Moreover, even when the event analyzed occurs only rarely, event history analysis has proven capable (in this study at least) of generating meaningful empirical results. This suggests that political scientists might be able to use EHA to undertake rigorous empirical testing of explanations of events generally considered too rare to be studied using multivariate analysis. For example, at the individual level, one might test explanations for a variety of forms of relatively rare political activity, such as engaging in protest activity or switching party identification. Students of international relations might use EHA to study such events as wars or treaties, and comparativists might test explanations of the conditions under which military coups are likely to occur.

Appendix

The Dependent Variable

The dates of lottery adoptions by states provides the information necessary to measure ADOPT in equation 1. The following states had adopted a lottery by 1986: New Hampshire 1964; New York 1967; New Jersey 1971; Massachusetts, Michigan, and Pennsylvania 1972; Maryland 1973; Illinois, Maine, Ohio, and Rhode Island 1974; Delaware 1975; Ver-

mont 1978; Arizona 1981; Connecticut and Washington 1982; Colorado 1983; California, Missouri, Oregon, and West Virginia 1984; Iowa 1985; Florida, Idaho, Kansas, Montana, and South Dakota 1986.

Indicators Used To Measure Independent Variables

Fiscal Health (FISCAL). Ratio of total-state-revenue-minus-total-state-spending to total state spending.

Degree to Which Single Party Controls Institutions of Government (PARTY). A dichotomous variable taking the value one if the governor and both legislative houses are controlled by the same party, zero otherwise. It cannot be measured for Minnesota and Nebraska in years with nonpartisan legislatures, so these cases are deleted from analyses involving PARTY.

Proximity to Elections. ELECT1 is a dichotomous variable taking the value one in the year of a gubernatorial election, zero otherwise. ELECT2 is a dichotomous variable taking the value one if it is neither an election year nor the year after an election, zero otherwise.

Personal Income (INCOME). State per capita income divided by the implicit price deflator for personal consumption expenditures, to convert per capita income to "constant" 1982 dollars.

Previous Adoptions by Neighbors (NEIGHBORS). Number of neighboring states that have adopted the lottery prior to the year of measurement. States are assumed to be neighbors of all states that share a border. (In addition, the pairs New Jersey and Maryland, and Massachusetts and Maine are treated as neighbors). The following lists the 48 states and their neighbors: Alabama has for neighbors MS, TN, GA, FL; Arizona has CA, NV, UT, CO, NM; Arkansas has LA, TX, OK, MO, KY, TN,

MS; California has OR, NV, AZ; Colorado has NM, AZ, UT, WY, NE, KS, OK; Connecticut has NY, MA, RI; Delaware has MD, PA, NJ; Florida has AL, GA; Georgia has FL, AL, TN, NC, SC; Idaho has WA, OR, NV, UT, WY, MT; Illinois has WI, IA, MO, KY, IN, MI; Indiana has KY, IL, MI, OH; Iowa has MO, NE, SD, MN, WI, IL; Kansas has OK, CO, NE, MO; Kentucky has TN, AR, MO, IL, IN, OH, WV, VA; Louisiana has TX, AR, MS; Maine has NH, MA; Maryland has VA, WV, PA, DE, NJ; Massachusetts has RI, CT, NY, VT, NH, ME; Michigan has WI, IL, IN, OH; Minnesota has ND, SD, IA, WI, MI; Mississippi has LA, AR, TN, AL; Missouri has AR, OK, KS, NE, IA, IL, KY, TN; Montana has ID, WY, SD, ND; Nebraska has KS, CO, WY, SD, IA, MO; Nevada has CA, OR, ID, UT, AZ; New Hampshire has MA, VT, ME; New Jersey has DE, PA, NY, MD; New Mexico has AZ, UT, CO, OK, TX; New York has PA, NJ, CT, MA, VT; North Carolina has SC, GA, TN, VA; North Dakota has SD, MT, MN; Ohio has KY, IN, MI, PA, WV; Oklahoma has TX, NM, CO, KS, MO, AR; Oregon has CA, NV, ID, WA; Pennsylvania has DE, MD, WV, OH, NY, NJ; Rhode Island has CT, MA; South Carolina has GA, NC; South Dakota has ND, NE, WY, MT, MN, IA; Tennessee has NC, GA, AL, MS, AR, MO, KY, VA; Texas has NM, OK, AR, LA; Utah has AZ, NV, ID, WY, CO, NM; Vermont has NH, MA, NY; Virginia has NC, TN, KY, WV, MD; Washington has OR, ID; West Virginia has VA, KY, OH, PA, MD; Wisconsin has MN, IA, IL, MI; Wyoming has CO, UT, ID, MT, SD, NE.

Religious Fundamentalism (RELIGION). Percentage of state population adhering to fundamentalist religions in 1971. Since data are available only for 1971, we must assume that the percentage fundamentalist population is stable within states for the period 1964-85.

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Data Sources: FISCAL: *Statistical Abstract of the United States*, selected years. PARTY: gubernatorial data through 1984 from Congressional Quarterly, *Guide to U.S. Elections*, 2d edition (1985); legislative data through 1984 from data set graciously provided by James Garand of Louisiana State University; data for 1985 and 1986 from Council of State Governments, *State Elective Officials and the Legislatures* and *The Book of the States*. ELECT1 and ELECT2: data through 1984 from Congressional Quarterly, *Guide to U.S. Elections*; data for 1985 and 1986 from Council of State Governments, *The Book of the States*. INCOME: data through 1984 from James Garand; data for 1985 from *Statistical Abstract of the United States*; implicit price deflator from CITIBASE (maintained by the Economics Department of Citibank). RELIGION: data from Douglas Johnson, Paul Picard, and Bernard Quinn, *Churches and Church Membership in the United States* (Washington: Glenmary Research Center 1974).

Notes

An earlier version of this paper was presented at the sixth annual Political Methodology Conference in Minneapolis, July 1989; we are grateful to the participants for their insights. We are also indebted to Gary King for calling our attention to the literature on event history analysis and to Virginia Gray, Susan Hansen, David Lowery, and Lee Sigelman for helpful comments. Moreover, several conversations with Stanley Feldman helped improve the paper. Thanks go, too, to James Garand, who graciously provided some of the data used in our study, and to Anthony Gierzynski for his help in collecting other data.

1. A variant of this explanation suggests that both nearby and distant states are relevant. Gray's (1973) government interaction model assumes that policies diffuse across states as a result of free interaction of officials from states that have already adopted with officials from states that have not yet adopted (see also Feller and Menzel 1978).

2. Allison (1984) and Tuma and Hannan (1984) present and cite numerous examples of research explaining the occurrence of events as diverse as a job

change, a bankruptcy, a hospitalization, or a death.

3. However, in no state has the lottery proven capable of generating a large proportion of a state's revenue; all lotteries yield less than 5% of a state's general revenues from its own sources (Mikesell and Zorn 1986).

4. More precisely, these studies have used (1) some linear transformation of the year of adoption or (2) the order of adoption by states.

5. However, event history analysis can also be used when the event in question *can* be repeated by an individual (e.g., moving one's residence), in which case the size of the risk set is stable over time.

6. For many policies, we would also expect that the ideological orientation of government officials may influence their motivation to adopt a policy and consequently that the political party controlling a state's government would affect the probability of adoption. But with the lottery we do not expect that governments controlled by Democratic parties should be more (or less) likely to adopt than those controlled by Republican parties. This is because a lottery is likely to induce a mixed ideological response from both conservatives and liberals. For example, for liberal politicians the fact that a lottery will generate revenues that can be spent to increase public services may be offset by the highly regressive nature of its incidence (Suits 1977). Similarly, conservative politicians may see the lottery as a way of avoiding the need to increase a mandatory sales or income tax but be concerned about government promotion of gambling. Thus, there is little reason to include the party in control in a model of state lottery adoption.

7. Two quite different lines of reasoning also support this hypothesis. First, Wagner's Law (Wagner 1877) suggests that many public services (e.g., parks) are perceived as "luxury goods"; they are not consumed when personal income is low but are increasingly demanded as incomes rise and individuals' private needs are fulfilled (see also Berry and Lowery 1987; Mann 1980, 50-52). Assuming this is true, the demand for government services and hence the need for government revenues should increase with personal affluence, thereby enhancing the probability of a lottery adoption. Second, Filer, Moak, and Uze (1988) contend that states with a high proportion of their population in poverty should be less likely than wealthier states to adopt a lottery, since legislators representing poor districts should oppose a regressive lottery that would disproportionately tax their constituents.

8. For a discussion of the variation across state lotteries on this and other dimensions, see Mikesell and Zorn 1986.

9. Aldrich and Nelson (1984, 31-35) explain the role of the cumulative normal distribution in the probit model.

10. An alternative specification of equation 1 would be:

$$\begin{aligned} \text{ADOPT}_{i,t} = & \Phi (b_1 \text{FISCAL}_{i,t-1} + b_2 \text{PARTY}_{i,t} \\ & + b_3 \text{ELECT1}_{i,t} + b_4 \text{ELECT2}_{i,t} \\ & + b_5 \text{INCOME}_{i,t-1} + b_6 \text{RELIGION}_{i,t-1} \\ & + b_7 \text{D}_{i,t} \text{NEIGHBORS}_{i,t}, \end{aligned}$$

where $D_{i,t}$ equals zero if the year, t , is 1964 and one if t is 1965 or later. This formulation restricts the effect of neighboring states to 1965 and after, assuming on logical grounds that previously adopting neighboring states could not have been responsible for the first state's adoption of the lottery in 1964. But since, in our data set, $\text{NEIGHBORS}_{i,t}$ (and hence $[D_{i,t}] [\text{NEIGHBORS}_{i,t}]$) equals zero for all states in 1964, equation 1 and the equation in this note yield identical coefficient estimates.

11. The reliance on a probit specification for our model makes it so that our hypotheses predicting interaction are not fully distinct from hypotheses about coefficients for individual independent variables in equation 1. As long as the coefficients for the independent variables are nonzero, some interaction among the independent variables in influencing the probability of adoption is guaranteed.

12. In the probit model of equation 1, the independent variables are assumed to be linearly and additively related to an unmeasured continuous interval-level variable that might be conceived as the inclination to adopt a lottery. Thus, the interactions we find among independent variables are interactions in influencing the probability of adoption (constrained to be within the range from zero to one) rather than the inclination to adopt, which is unconstrained with respect to maximum and minimum.

13. For a justification of the precise values at which the independent variables are set (e.g., the value for FISCAL that represents "very good" fiscal health), see the notes to Table 2.

14. A state in the year after an election with moderate fiscal health and a very small fundamentalist population has only a .025 (= .026 - .001) greater probability of adoption than a state with a very large fundamentalist population.

15. We do not even attempt to assess Hypothesis 10, since Hypothesis 8—also about the effect of unified control of government—is disconfirmed.

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CONTRIBUTIONS, LOBBYING, AND COMMITTEE VOTING IN THE U.S. HOUSE OF REPRESENTATIVES

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Lobbying efforts and campaign contributions from coalitions of groups are used to explain representatives' voting decisions within the U.S. House Ways and Means and Agriculture Committees. Information about which groups worked together on two controversial issues and which representatives they lobbied was obtained through personal interviews and a mail survey of professional lobbyists. The analysis reveals that committee-level voting, particularly in the Ways and Means Committee, is best explained by the total number of lobbying contacts representatives received from groups on each side of the issue. Campaign contributions proved somewhat useful for explaining groups' lobbying patterns; but it appears to be lobbying, not money, that shapes and reinforces representatives' policy decisions.

Interest groups have played an increasingly visible role in the congressional policy process in recent years. Frequent coalitional activity, extensive grass roots lobbying, and growth in the numbers of citizen groups and political action committees have enhanced the presence of organized interests before Congress (Loomis 1983; Schlozman and Tierney 1986; Sorauf 1988; Walker 1983). The widespread participation of groups is unmistakable, but whether and how they influence congressional policy decisions is not so clear. Do groups' lobbying efforts make a difference in policy outcomes? Are groups' campaign contributions related to their lobbying activities, and if so, how? Do campaign contributors affect representatives' decisions independently of lobbying?

Political scientists have learned much in the past few years about lobbying activities (Berry 1989; Schlozman and Tierney 1986), coalitional patterns among groups (Salisbury et. al. 1987), and campaign contributions (Grenzke 1989a; Welch

1982; Wright 1985). Yet too often, these phenomena are studied separately. Most analyses of the influence of campaign contributions, for example, do not consider the effects of organized lobbying efforts that often accompany contributions. And most analyses of interest group coalitions do not assess the impact of *coalitional lobbying efforts* and campaign contributions on specific bills. My objective here is to integrate lobbying efforts and campaign contributions from coalitions of groups into an equation of group influence and to examine these factors where they are likely to matter most—in the low-visibility setting of congressional committees.

The analysis focuses on two controversial issues from the 99th Congress, one handled by the House Ways and Means Committee and the other by the Agriculture Committee. For each issue, I obtained information about which groups worked together and which representatives they lobbied through personal interviews and a mail survey of professional

lobbyists. Data on any campaign contributions from groups in each coalition to representatives on the relevant committees were obtained from the Federal Election Commission. I address two basic questions in the analysis: To what extent do representatives' voting decisions within committee depend on interest groups' coalitional lobbying efforts? and To what extent are interest groups' lobbying efforts a function of prior campaign contributions? Thus, rather than assume that money influences voting only directly through a quid pro quo, I consider whether money affects voting in a much more subtle and indirect fashion by influencing groups' choices of whom to lobby.

Campaign Contributions and Lobbying

Most organizations that sponsor political action committees (PACs) also maintain active lobbying operations; as a result, campaign contributions and lobbying often occur together. Sabato (1984, 124), in his survey of multicandidate political committees, found that 68% of the corporations, unions, and associations with PACs also have lobbying offices or representatives in Washington. Because contributions and lobbying are often complementary activities, the effects of either or both on congressional voting must be analyzed jointly. Studies claiming that contributions have a significant influence on congressional voting but failing to separate contributions from lobbying (e.g., Brown 1983; Frendreis and Waterman 1985; Saltzman 1987; Wilhite 1988; Wilhite and Theilman 1987) may overstate the influence of contributions. Grenzke (1989b, 10), for example, suggests that contributions are probably a "surrogate measure of an interest group's efforts to lobby and mobilize votes in the district."

Even without controlling for lobbying, many analyses demonstrate little overall influence of contributions (e.g., Chappell

1982; Evans 1986; Grenzke 1989b; Keim and Zardkoochi 1988; Welch 1982; Wright 1985). Contributions sometimes exert slightly stronger effects on less visible issues and in less visible settings (e.g., Jones and Keiser 1987; Schroedel 1987), but even in these situations the effects are weak. The total reported effects of campaign contributions on congressional voting, therefore, are either generally quite small or else indistinguishable from organizational efforts other than campaign contributions.

The sheer magnitude of PAC expenditures, however, and the visibility and public concern about PAC money suggest that the importance of campaign contributions in the legislative process should not be dismissed quickly. Contributions may not influence voting directly, but they may affect lobbying activities. And lobbying activities, at the very least, help to define and to shape legislative agendas and debates.

Most scholars and political practitioners acknowledge some connection between PAC money and lobbying. It is widely believed, for example, that contributions—even if they do not "buy" votes—"buy" access for subsequent lobbying efforts (e.g., Berry 1984; Conway 1986; Dunn 1972; Gopoian 1984; Herndon 1982; Langbein 1986; also Elizabeth Drew, "Politics and Money," pt. 1, *New Yorker*, 6 December 1985, pp. 54-149). What "access" means to lobbyists is not exactly clear; but at a minimum it seems that contributions secure "preferential treatment"—an advantage in making appointments and getting phone calls returned (Sabato 1984, 127; also Drew, "Politics and Money," p. 130). In addition to creating opportunities for presenting one's point of view, contributions may also be important for amplifying lobbyists' messages. Representatives may "hear you better," for example, when a contribution precedes lobbying. Even influential organizations, organizations that gen-

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erally have little trouble scheduling appointments, may find contributions useful for enhancing their messages.

Finally, organizations that do not sponsor political action committees may still deploy their lobbying efforts on the basis of how much PAC money representatives have received from groups with similar and opposing views. Since PAC contributions are often rewards for past support rather than inducements for future support, a representative's financial constituency provides important information about the representative's general policy orientation. Strong financial support, or lack of support from particular groups may signal the extent to which a representative can be persuaded, and this information may in turn affect groups' decisions about whether or not to lobby.

While PAC contributions might affect lobbying in several ways, does lobbying itself affect representatives' voting choices? If contributions determine the lobbying patterns that representatives observe and if representatives respond to lobbying efforts, groups need not "buy" votes directly in order for contributions to be important. A substantial body of literature suggests that lobbying efforts do not persuade legislators at all, but merely reinforce or encourage those who already agree with them (Bauer, Pool, and Dexter 1963; Matthews 1960; Milbrath 1963; Scott and Hunt 1966). Taking a "communications perspective," Milbrath argues that representatives' policy predispositions determine the lobbying patterns they observe and that lobbying merely reflects preferences formed on the basis of other considerations such as party, constituency, and ideology. Bauer, Pool, and Dexter argue that the lobbyist is merely a "service bureau" for "congressmen already agreeing with him" (1963, 353). And Matthews concludes, "Lobbying changes relatively few votes. Indeed, not much lobbying is intended to do so. The principal effect of lobbying is not

conversion, but reinforcement" (1963, 191).

This view, of course, contrasts with earlier perspectives by Truman (1951), Lathman (1952), and others who argued that legislators adjust their policy positions to satisfy group demands. More recently, scholars have reached conclusions falling somewhere between these two perspectives. Berry (1977) and Smith (1984), for example, claim that groups are sometimes successful in persuading marginal, or "swing," legislators. Denzau and Munger (1986) demonstrate that legislators are most likely to respond to organized group pressures when unorganized constituents are uninformed or indifferent about policy alternatives. Fowler and Shaiko (1987) and Langbein and Lotwis (1988) have shown empirically that grass roots lobbying is important on some issues. Richard Smith (1988) discovered that lobbying often encourages legislators to commit their support to groups, and Rothenberg (1989) has found evidence that some public interest lobbying influenced representatives' votes on the B1 bomber.

Judging from the millions of dollars that organized interests spend lobbying members of Congress each year, these results should not be at all surprising. Reported expenses on lobbying during 1985 (the first session of the 99th Congress) exceeded 49 million dollars; and during 1986, total reported expenses were 60.9 million dollars.¹ Thus, the two-year total—nearly 110 million dollars—is by comparison roughly the same magnitude as total contributions from all political action committees during a two-year election cycle. During the 1983–84 election cycle, for example, contributions to congressional candidates from all connected and nonconnected multicandidate political committees totaled 105.3 million dollars; and during the 1985–86 election cycle, PAC contributions totaled 132 million dollars. Considering that organi-

zations are not required to report expenses for grass roots lobbying, actual lobbying expenses may be up to ten times larger than reported expenditures (Pressman 1983).

Exactly what the role and influence of organized group efforts are in the legislative process are clearly important but largely unexplored questions. How and to what extent do campaign contributions shape the lobbying patterns that representatives observe? How important are representatives' partisan and ideological predispositions for determining the lobbying patterns they observe? Does lobbying exert an independent and significant influence on representatives' voting choices? I begin to investigate these questions using the data and methods described below.

Issues and Groups

Issues

I analyze lobbying and contributions on two pieces of legislation from the 99th Congress. One concerns tax provisions for the reauthorization of the Comprehensive Environmental Response, Compensation, and Liability Act ("superfund"), first passed in 1980, which reached the House Ways and Means Committee in October 1985. While there were many difficult and controversial aspects of the superfund renewal in general, controversy about who would pay for hazardous waste cleanup was one of the most difficult. I focus on just this one aspect. The question of who would pay split the business community between oil and chemical producers on the one hand and a broad base of general businesses on the other. The coalition of general business groups, headed by the Grocery Manufacturers Association opposed a plan (passed earlier by the Senate) to impose a tax on the sale and manufacture of virtually all raw materials. This tax, called a broad-based tax (but called a

value-added tax by opponents), was estimated to affect some 30 thousand businesses. The alternative (status quo) plan was to concentrate the tax burden on the oil and chemical industry and was estimated to affect only about a thousand businesses. Understandably, the oil and chemical groups opposed this plan vigorously.

The second issue concerns the wheat and feed grains provisions of the 1985 farm bill. The House Agriculture Committee considered a variety of proposals in 1985 to make grain prices more competitive on the world market yet maintain income guarantees for individual farmers. In broad terms, the proposals included freezing both commodity loan rates and target prices at current levels, requiring farmers to pay back their commodity loans to the government at the going market prices for grain rather than defaulting on their loans, and holding a national referendum in which farmers themselves would decide whether or not to implement substantial production cutbacks in exchange for higher federal subsidies.² The committee finally settled on a moderate plan advanced by Representative Foley, chairman of the Subcommittee on Wheat, Soybeans, and Feed grains, to allow farmers to continue to default on their loans but to reduce loan rates gradually by setting them at 75-85% of average domestic market prices over a five-year period (excluding the high and low years). Target prices were to remain fixed for two years, but then be adjusted to the market.

The Foley plan received the general backing of the American Farm Bureau Federation, national commodity groups (the National Corn Growers Association, American Soybean Association, and National Association of Wheat Growers), and grain users (e.g., the American Bakers Association, National Cattlemen's Association, American Meat Institute, and Continental Grain Company). Op-

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posing the Foley plan but generally supporting the referendum and proposals to freeze loan rates and target prices at current levels were grass roots farm organizations (e.g., the National Farmers Union, National Farmers Organization, and American Agricultural Movement).

These two issue areas were selected for analysis after carefully reviewing the major legislative efforts of the first session of the 99th Congress, the most recently completed session at the time initial interviewing began. Each issue scored high on several important criteria for assessing the direct and indirect effects of lobbying and campaign contributions from coalitions of groups. First, each issue involved a manageable set of alternatives that generated relatively well defined coalitions of interests on each side; yet each was also sufficiently controversial and important to stimulate widespread participation from a broad cross section of groups. Second, the issues involved two distinct committees (an "influence" committee and a "constituency" committee [Fenno 1973]), both of which deal regularly with organized group pressures. Accommodation of group concerns was important for both committees not only to control the formulation of policy within committee rather than on the floor but also to satisfy reelection constituencies at home; yet the fundamental differences between these two committees may illuminate aspects of the lobbying process that are specific to certain kinds of committees. Third, each issue involved several recorded votes within committee, all of which escaped substantial media attention. Low media visibility makes it more likely that legislators would and could respond to campaign contributions and interest group lobbying (e.g., Evans 1986). Just one article appeared in the *Washington Post*, none in the *New York Times*, in the week after the controversial decision within the Ways and Means Committee to report a broad-based tax; and neither paper

covered the issue in the week preceding the markup (see, e.g., Jones and Keiser 1987). The Agriculture Committee's decision on wheat and feed grains received no mention in either paper in the weeks before or after the markup.

These two policy areas are not necessarily representative of all issues considered by the 99th Congress. But then it is difficult to conceive of a single, representative issue. The issues selected for study here were chosen *because* they attracted considerable interest group activity and were good candidates for observing group forces at work. If group forces are at work in the congressional process, they should be observable on these issues. The disadvantage, of course, is that one cannot be as confident that strong evidence of group influence is a general phenomenon on all issues. But given the lack of *systematic* evidence that groups ever successfully influence policy choices (despite considerable circumstantial evidence of their influence), the analysis of issues most likely to exhibit strong effects promised a greater return on the research investment.

Groups

Identifying groups associated with particular policies or policy areas is a difficult research task, as evidenced by the elaborate efforts of Salisbury and his associates (1987). The organizations involved with the two issues selected here were identified through a variety of methods.³ Drawing from the published reports of committee and subcommittee hearings, relevant subject classifications in *Washington Representatives* (Close 1985), and coalitional lists obtained from Washington lobbyists, I assembled an initial list of more than three hundred potentially active organizations across both issues. Each of these groups was contacted by telephone in an effort to identify one particular individual who was instrumental in the group's lobbying effort. The first

round of contacts eliminated roughly one-fourth of the initial sample—organizations that claimed not to have taken a position on either of the issues. I conducted a preliminary round of open-ended interviews in Washington with lobbyists from the leading organizations on each issue. From these personal interviews I developed a questionnaire and mailed it directly to each of the active lobbyists identified in the first round of phone contacts. A second wave of questionnaires was mailed a month later to organizations not responding on the first wave.

Questionnaires were returned from 97 different organizations on both issues, yielding an overall response rate of nearly 40%. Not all of these questionnaires were usable for the present analysis, however, as many either did not complete the section of the questionnaire dealing with lobbying activities or indicated that their involvement was not targeted at specific representatives on either committee.⁴ A total of 35 different organizations reported to have lobbied one or more members of Ways and Means on the tax issue; and of these, 24 groups supported the broad-based tax plan, and 11 opposed it. The sample contains a diverse mix of corporations and national associations.⁵ There is no guarantee, however, that the sample is representative of all organizations that lobbied on the issue. Because groups that supported the broad-based tax were a narrower, more homogeneous set of groups, they were easier to identify. Thus, their greater representation in the sample may simply reflect the fact that they were surveyed at a higher rate.⁶ Nevertheless, both coalitions are well represented. Of the 35 organizations 12—8 supporting and 4 opposing the broad-based tax—sponsored PACs during the 1983–84 election cycle and made contributions to members of the Ways and Means Committee. This rate of PAC sponsorship—about 1 in 3—is similar to what Gais (1983) found in a much larger,

randomly selected sample of organizations with Washington representation.

For the agricultural issue, a total of 31 organizations provided lobbying information, of which 23 generally supported the Foley plan on wheat and feed grains and eight opposed it. This sample includes general membership organizations, commodity groups, producer groups, corporations, unions, and state organizations.⁷ Of the 31 organizations 14 sponsored PACs during the 1983–84 election cycle and made contributions to members of the Agriculture Committee. This sample does not include all organizations involved with the wheat, feed grains, and soybean provisions of the 1985 farm bill; but it does include most of the major players on both sides of the issue.

Statistical Model

Most lobbying efforts are coordinated efforts. Groups seldom act alone, in isolation from other groups. Not only do interest groups tend to work through coalitions; but when these coalitions form, they usually form on *both* sides of the issue (Bosso 1986; Gais, Peterson, and Walker 1984; Light 1985; Loomis 1986; McFarland 1987; Ornstein and Elder 1978; Salisbury et. al 1987). To reflect the tendency for representatives to observe the combined lobbying efforts from multiple groups on both sides of an issue, I include two separate lobbying variables in the statistical model. The variable *lobby_f* denotes the number of groups lobbying a representative *for* passage of a bill, and *lobby_a* denotes the number of groups lobbying *against* passage.

I assume representatives' votes are a function of *lobby_f* and *lobby_a*, but that each of these influences is itself a function of other variables. In particular, I propose that *lobby_j* ($j = f, a$) depends on the representative's party affiliation (*party*), personal ideology (*ideology*), relevant eco-

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nomic characteristics of his or her constituency (*constituency*), whether or not the representative is a committee leader (*leader*), and two campaign contribution variables. The variable *contributors*, ($j = f, a$) is the number of groups for or against passage that contributed to the representative's last election campaign; and ($money_f - money_a$) is the difference in total contributions from all groups in the sample (whether they lobbied the representative or not) that took a position for and against passage. The functional relationships between these explanatory variables and lobbying are specified in equations 1 and 2, where both functions are assumed to be linear and where u and v are stochastic terms.

$$\text{Lobby}_f = f[\text{party, ideology,} \\ \text{constituency, leader, contributors}_f, \\ (\text{money}_f - \text{money}_a), u]. \quad (1)$$

$$\text{Lobby}_a = g[\text{party, ideology,} \\ \text{constituency, leader, contributors}_a, \\ (\text{money}_f - \text{money}_a), v]. \quad (2)$$

I expect the contribution variables to exert direct effects on lobbying.⁸ Thus, the greater the total number of groups contributing money to a representative (i.e., contributors), the greater should be the number of groups lobbying; and this should hold for groups on both sides of the issue. Also, the difference in the total amount of contributions a representative has received from competing groups (i.e., $money_f - money_a$) should directly affect lobbying decisions. Assuming that campaign contributions are often rewards for past behavior, contributions convey information about representatives' general policy orientations. Representatives who generally favor corporate interests over labor interests, for example, are more likely to receive more total money from corporate PACs than from labor PACs. Since information about the balance of

money a representative has received from PACs with competing interests is readily available to lobbyists, they can use this information to judge whether or not a legislator is likely to be hostile, friendly, or neutral. If groups are more likely to lobby their legislative friends (Dexter 1969), then the greater the balance of contributions in favor of groups supporting passage, the greater should be the number of groups also lobbying in support of passage and the lower the number of groups lobbying against passage.

Finally, I assume in equations 1 and 2 that decisions to lobby by groups on opposing sides of the issue are made independently; that is, lobby_f does not depend on lobby_a , and vice versa. This is a reasonable assumption if groups base their lobbying decisions primarily on jointly available exogenous cues such as partisanship and constituency and not on expectations about what their opponents might be doing. Unfortunately, there is little or no available theory about whether or when groups might lobby in order to counteract the influence of other groups. Thus, I have adopted the simplest assumption as a first approximation to understanding the impacts of money and lobbying.⁹

Whether campaign contributions affect representatives' voting decisions directly or whether they influence voting only indirectly through lobbying is unclear. In equation 3 voting is posited to be a direct function of contributions and lobbying, together with other exogenous variables.¹⁰

$$\text{Votes} = h_1 [\text{party, ideology,} \\ \text{constituency, contributors}_f, \\ \text{contributors}_a, \text{money}_f, \text{money}_a, \\ \text{lobby}_f, \text{lobby}_a, e_1]. \quad (3)$$

Because the effects of contributions and lobbying may be different for the two coalitions, separate measures of contrib-

uting and lobbying from each of the coalitions is included in this equation. Also, both measures of contributions are included: the total dollar amount of contributions from each coalition ($money_f$ and $money_a$) and the number of contributors from each coalition ($contributors_f$ and $contributors_a$). Representatives might vote to satisfy either the coalition providing the largest total amount of money, or the coalition with the largest number of contributors. The number of contributors to whom a representative must explain his or her vote may be as important as (or perhaps even more important than) the actual dollar amounts involved (e.g., Smith 1988).

An alternative theoretical specification to equation 3 is equation 4, where contributions do *not* directly influence representatives' voting decisions.

$$\text{Votes} = h_2[\text{party, ideology, constituency, lobby}_f, \text{lobby}_a, e_2]. \quad (4)$$

According to equation 4, instead of weighing the number of contributing groups in each coalition or the total dollar amounts contributed, representatives directly respond only to the lobbying efforts by each coalition. Lobbying efforts themselves, however, may depend on prior campaign contributions (equations 1 and 2). Indeed, many popular claims, as well as considerable empirical evidence, suggest that money affects voting only indirectly through the access groups gain to legislators. Thus, the specification of equation 4 is not only highly plausible on theoretical grounds but also serves to identify the voting equation for estimation.

One potential problem in estimating either voting equation is that estimates of lobbying will be biased if the error term, e , is correlated with the errors of equations 1 and 2. This may happen if groups receive and use inside, or "private," information about voting decisions that is not captured by measures of ideology, parti-

sanship, and constituency. If, for example, a representative has decided to vote against a group as part of a logrolling arrangement with another legislator and privately informs the group of this fact, the systematic portions of neither the voting nor the lobbying equation will capture this effect.¹¹ The information received by the group will be incorporated into the error term for lobbying, and the information committed by the representative will be incorporated into the error term for voting. Consequently, it is possible that $E(e, u) \neq 0$ and $E(e, v) \neq 0$.¹²

Assuming that $E(e, u) \neq 0$ and $E(e, v) \neq 0$, equation 3 can be estimated through Two-Stage Least Squares (2SLS), where *votes* in the second stage is regressed on predicted values of $lobby_f$ and $lobby_a$ from the first stage. In the system of equations 1, 2, and 3, the lobbying equations—1 and 2—are just identified; and the voting equation—equation 4—is over-identified. The lobbying equations can be estimated through Ordinary Least Squares (OLS) because none of the right-hand-side variables in equations 1 and 2 appear as dependent variables in other equations.

Equation 3, however, cannot be estimated through 2SLS, because equations 1, 2, and 3 together fail to satisfy the order condition. The voting equation—equation 3—excludes only the exogenous variable leader but includes two endogenous variables ($lobby_f$ and $lobby_a$). Nevertheless, estimation of equation 4 through OLS is possible and will reveal the importance of contributions for voting with lobbying controlled even though the coefficient estimates for the lobbying variables may be biased. I present results from the estimation of both voting equations—3 using OLS, and 4 using 2SLS.

Operationalization of the Variables

Independent Variables. The number of lobbying contacts observed by each repre-

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sentative from groups supporting and opposing each proposal was measured simply by summing together the number of organizations in each coalition that reported lobbying that representative. (The coalition of groups for and against each proposal was defined simply as the set of groups in the sample that took a position for or against.) Obviously, what it means to "lobby" a representative is an imprecise notion. The concept of lobbying can refer to a diverse set of activities, ranging from testifying at a committee hearing to generating letters and phone calls from the grass roots to meeting personally with representatives and their staffs. Moreover, groups lobby with varying degrees of intensity and with varying levels and mixes of resources. Ideally, one would collect data on each group's intensity of effort across a whole range of activities for each representative. But pragmatically, this is hardly feasible even for representatives on just one committee. Consequently, the survey asked lobbyists first to indicate the various kinds of lobbying activities in which their organization participated in general and then to indicate how much effort the organization expended overall on those activities for particular members of either committee. (The complete question on lobbying effort is reported in the Appendix). Groups that reported "some" or a "great deal" of effort were coded as having lobbied, others as not having lobbied.

Measuring coalitional lobbying in this way implies that all groups are weighted equally in the representative's calculus. Certainly, the pure size of the coalition observed by the representative is an important factor; but some groups may be more important in the representative's deliberation than others, and, ideally, these should be weighted accordingly. It is by no means obvious, however, exactly what the appropriate weighting scheme should be. Perhaps groups with strong grass roots connections carry the most

weight in the representative's eye; or perhaps groups with influential Washington contacts and large research staffs are most important. Maybe new groups or groups that the representative seldom hears from gain special notice. Another possibility is that representatives assess a group's lobbying effort not in terms of total visits, letters, and so forth but in terms of the group's effort relative to the resources it has available. All of these are plausible weighting schemes, and all could yield very different overall measures of coalitional lobbying. Establishing exactly what the appropriate weighting scheme should be is an important and complex research issue but also one that lies outside my purview. I begin here with the most simple hypothesis, namely, that the pure size of the coalition matters. If the simple number of lobbying contacts makes little difference in the representative's vote choice, it is unlikely that additional refinements in weighting will make much difference. Once the utility of this simple weighting scheme has been investigated, experimentation with more complex schemes should be attempted.

The remaining independent variables specified in equations 1 and 2 are defined as follows. Party is coded zero for Democrat and one for Republican. Ideology is measured by the representative's Americans for Democratic Action score from the previous Congress (the 98th).¹³ Constituency is measured by the total value added (in millions of dollars) by production of petroleum and chemical products within the district and by millions of bushels of wheat, corn, and soybeans produced within the congressional district.¹⁴ Contributors_{*j*} (*j* = for or against) is the total number of groups with positions for or against that made a campaign contribution to the representative during the 1983-84 election cycle; and the total dollar amount of contributions each representative received from groups sup-

porting and opposing the various proposals (money_j, with j = for or against) is computed as the sum of contributions from groups in each coalition made during the 1983–84 election cycle. Leader is a dummy variable coded one if a representative is the committee chairman, a subcommittee chairman, or ranking minority member, otherwise zero.

Committee Votes. The Ways and Means Committee markup on the tax plan lasted for nearly 10 hours, during which time 13 different motions and amendments were considered. Of these 5 were either tabled or decided by voice vote or a show of hands, and the remaining 8 were decided by a recorded vote.¹⁵ I have chosen three of the recorded votes for analysis here. These three votes took place on the major amendments and substitutes considered by the committee. The working vehicle was the Ways and Means staff proposal, which generally recommended a continuation of the concentration of revenue burden on the chemical and petroleum groups. Representative Jones (D-OK), the legislative leader on the committee for the petroleum and chemical interests, proposed an amendment to the staff plan that called for a broad-based tax virtually identical to that passed earlier by the Senate. After a few minor perfecting amendments, the first recorded vote of the markup was taken on the Jones amendment. It failed by an 18–18 vote. (Hereafter, I refer to this vote as the Jones Amendment.) Representative Downey (D-NY) then proposed an amendment raising the petroleum and chemical feed stock taxes and also deleting additional borrowing authority as recommended in the staff proposal. Jones countered with a substitute to Downey's amendment calling again for a broad-based tax, along with some minor modifications. This substitute by Jones (hereafter, the Jones Substitute) failed by a 17–18 margin. Representative Fowler (D-GA) then offered another substitute to Downey's amend-

ment containing a pared-down version of Jones's broad-based tax, together with elimination of Downey's proposal to delete borrowing authority from the staff proposal. Fowler's compromise plan (hereafter, the Fowler Substitute) gained support from other Democrats on the committee and passed by an 18–17 vote. This plan was eventually reported by the committee but not until Chairman Rostenkowski, who opposed the broad-based tax, extracted an agreement to reconsider the committee proposal under an open rule on the House floor. For each of the three committee votes, a *yea* vote (coded one) on each amendment was a vote in favor of a broad-based tax.

The Wheat, Soybeans, and Feed Grains Subcommittee had great difficulty settling on a single plan and ultimately forwarded several options to the full committee. The committee considered these options in markup sessions on 23–25 July 1985.¹⁶ I analyze voting in the full committee on two key proposals for which groups' positions could be clearly identified. One of these proposals was in the form of a substitute amendment, offered by Representative English (D-OK), to the Foley compromise plan, which served as the first amendment to the text of HR2100 (the 1985 General Farm Bill). The English Substitute called for a freeze on loan rates and target prices for the life of the bill but required farmers to pay back their commodity loans at market prices in order to be eligible for full deficiency payments. The English Substitute was defeated by a 19–22 margin with two abstentions. Once the English Substitute was defeated, the battle was essentially over; for the Committee was left with no alternatives at that time except to return the issue to the subcommittee (which previously could not reach agreement on a single plan) or to report the bill to the floor without specific recommendations for wheat and feed grains. Hence, I analyze voting on the English Substitute rather than the Foley Amendment itself.¹⁷ The committee con-

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vened the following day and approved the Foley Amendment by a 26-15 margin.

The second vote I include in the analysis took place on the third day of the markup on an amendment offered by Representative Stenholm (D-TX) to strike the referendum option sent forward by the subcommittee from the committee report. The committee approved the Stenholm Amendment, striking the referendum provisions, by a 24-17 margin. A *yea* vote (coded one) on the English Substitute was a vote in favor of a minimum loan rate and against the market-oriented plan; and a *yea* vote (coded zero) on the Stenholm Amendment was a vote against the referendum concept.

I computed a composite measure of voting (*votes*) in both committees by summing representatives' votes on the sepa-

rate measures. The variable *votes* ranges from zero to three for votes within the Ways and Means Committee, with larger values reflecting stronger support for the broad-based plan. For voting within the Agriculture Committee, *votes* ranges from zero to two, with higher values indicating stronger support for the English plan and the referendum concept.

Results

Lobbying

Reported in Tables 1 and 2 are the OLS estimates for the lobbying equations. In Table 1, recall that the dependent variable, *lobby_i*, is the number of groups lobbying for the broad-based tax (i.e., for the

**Table 1. OLS Estimates of Coalitional Lobbying,
Ways and Means Committee**

Variable	Number of Groups Lobbying	
	For a Broad-based Tax	Against a Broad-based Tax
Intercept	12.82 (5.42)	.92 (.86)
Party (0 = D, 1 = R)	-3.38* (-1.62)	1.97** (2.22)
Ideology (ADA voting score)	-.10** (-3.31)	.04** (3.04)
Constituency (district chemical production)	.30 (.48)	.23 (.83)
Leader (1 = leader, 0 = rank and file)	3.16** (2.35)	1.37** (2.37)
Contributors	.69* (1.45)	.44* (1.39)
Money _f - money _a	-.23 (-.58)	-.26* (-1.78)
Number of observations	36	36
R-squared	.51	.39
Corrected R-squared	.40	.36

Note: t-ratios are given in parentheses.

**p* < .10.

***p* < .05.

**Table 2. OLS Estimates of Coalitional Lobbying,
Agriculture Committee**

Variable	Number of Groups Lobbying	
	For a Broad-based Tax	Against a Broad-based Tax
Intercept	2.28 (6.65)	7.62 (6.65)
Party (0 = D, 1 = R)	-.89** (-2.06)	-.28 (-.22)
Ideology (ADA voting score)	-.03 (-.46)	-.03* (-1.52)
Constituency (district grain production)	.001 (1.11)	.01* (1.70)
Leader (1 = leader, 0 = rank and file)	-.06 (-.15)	.59 (.55)
Contributors	.22* (1.40)	.42 (1.21)
Money _f - money _a	.08 (.64)	-.04 (-.07)
Number of observations	43	43
R-squared	.25	.23
Corrected R-squared	.13	.10

Note: t-ratios are given in parentheses.

* $p < .10$.

** $p < .05$.

Jones Amendment and the Jones and Fowler Substitutes); and lobby_a is the number of groups lobbying against the broad-based tax. For the agricultural lobbying (Table 2), lobby_f is the number of groups that supported English and opposed Stenholm; and lobby_a is the number of groups that opposed English and supported the Stenholm Amendment.

Judging from the coefficients that attain statistical significance in Tables 1 and 2, party and ideology are the most consistent predictors of the numbers of groups lobbying in both committees. For the superfund tax issue, the representatives lobbied most heavily by organizations that supported a broad-based tax tended to be conservatives and Democrats, and those lobbied most heavily by groups opposed to the broad-based tax tended to be liberals and Republicans. Although the

tax issue was not a strong partisan or ideological issue, conservatives initially were somewhat more sympathetic to the broad-based tax than were liberals.¹⁸ For the agricultural issue, conservative Democrats were most often lobbied by the coalitions on both sides of the English-versus-Foley-and-Stenholm controversy. These representatives tended to hold the swing votes. Conservative Republicans generally supported the Foley plan and opposed the referendum, whereas liberal Democrats were supported by English and the referendum.

Leadership is an important determinant of lobbying in the Ways and Means Committee, as Chairman Rostenkowski, the six subcommittee chairs, and the ranking minority member (Duncan-TN) were lobbied by more groups on both sides of the issue than were nonleaders. Leaders of the

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Agriculture Committee, in contrast, were *not* more frequent lobbying targets by either coalition. Representatives' constituency characteristics, however, are somewhat better predictors of lobbying by the agricultural coalitions than by the superfund coalitions.¹⁹ As one might expect, district grain production is more relevant to the agricultural coalition that includes the commodity groups.

Finally, for both committees, the number of groups lobbying depends—albeit weakly—on the number of groups making campaign contributions. With the exception of the more conservative groups before the Agriculture Committee, the greater the number of groups that contributed, the greater the number of groups that lobbied. This relationship, although weakly significant in a statistical sense, is evident nonetheless, suggesting that a campaign contribution may indeed facilitate access or amplify lobbying messages. Groups' lobbying decisions, therefore, are not solely a function of representatives' predispositions as reflected by partisanship and ideology. Despite the obvious importance of party and ideology for shaping lobbying patterns, campaign contributions also provide an independent and significant basis for lobbying.

Much weaker statistically are the differences in aggregate contributions from opposing coalitions. A financial advantage significantly encouraged lobbying only by those groups opposed to a broad-based superfund tax. The other three coalitions were not significantly encouraged or discouraged from lobbying by an advantage or disadvantage in aggregate contributions. Lobbying, therefore, is barely affected by differences in aggregate contributions between the two coalitions. Groups do tend to lobby their partisan and ideological allies (e.g., Dexter 1969), particularly on a committee like Ways and Means; but once these factors are controlled, aggregate contribution patterns convey little information about rep-

resentatives' policy preferences that systematically affects lobbying activity.

While lobbying does depend to some extent on whether or not contributions have been made, the relationship is not as strong as might be expected. One possibility for why the relationship between money and lobbying is not stronger is that only a fraction of the organizations included in the analysis sponsored PACs during the 1983–84 election cycle. These organizations may have conditioned their lobbying activities on their campaign contributions, but the effects could be masked by dominance of organizations that did not make contributions and may not have conditioned their lobbying on other groups' contributions. Given this possibility, I estimated the effects of contributions on lobbying at the level of individual groups. For the 24 organizations that lobbied and made campaign contributions across both issues, I derived probit estimates of whether or not the group lobbied a representative as a function of the size of the contribution the group made to the representative, controlling for party, ideology, constituency, and leadership. Of the 24 individual-level equations, 8 yielded statistically significant relationships between contributions and lobbying. Thus, the individual-level analyses yield roughly the same conclusions as the aggregate analysis. Political action committee contributions encourage lobbying to some extent, but organizations base their lobbying decisions on other factors as well.

There are interesting and important differences between the two committees in terms of lobbying. In general, lobbying patterns in the Ways and Means Committee are much more structured and predictable than in the Agriculture Committee. In Ways and Means, groups are more likely to lobby their partisan and ideological allies, to lobby the committee leaders, and to lobby representatives to whom they have made campaign con-

tributions. These patterns suggest that channels of access are more structured—and probably more restricted—in Ways and Means, as groups are relatively cautious and tend to stick close to their legislative friends, particularly those to whom they have given money.

One explanation for this tendency is that Ways and Means encounters a larger and more diverse array of organized groups than does the Agriculture Committee. Smith and Deering (1984) report that more than five times as many bills are referred to Ways and Means than to Agriculture. They observe (1984, 65) that the focus in Ways and Means on just taxation issues "obscures the vast number of separate interests affected by the tax code." Thus, any organization concerned about taxes—and surely this includes almost all groups at one time or another—will come in contact with the Ways and Means Committee. Manley (1970, 324), for example, asserts that few committees attract the attention of organized groups like Ways and Means.

The Agriculture Committee, which is not well known for its jurisdictional fragmentation (Smith and Deering 1984), tends to deal with the same set of interests on issue after issue. The commodity groups, the livestock producers, the general farm organizations, the agribusiness interests, and so forth interact with members of Agriculture on a continual and regular basis.²⁰ Groups and members have ample opportunity to get to know one another well. Members of Ways and Means, however, have less opportunity to develop working relationships with the many groups that come before them. Clearly, not all organizations lobby on tax matters each year. Thus, not only the difference in the number of groups that come before each committee but also the frequency with which those groups appear before the committees make access to Ways and Means more difficult and more uncertain.

Furthermore, compared with their colleagues on the Agriculture Committee, members of Ways and Means are more senior, generally safer electorally, and—at least from their perspective—concerned with broader and more important substantive problems. As a result, policy conflict is much higher in Ways and Means (Smith and Deering 1984), often making for sharper ideological and partisan distinctions that impose additional constraints on which members groups work with (Fenno 1973, 56). In short, differences in the policy environments of the two committees suggest that access should be more fluid, less structured, and less formal in Agriculture than in Ways and Means. As a result, campaign contributions and members' ideological and partisan ties may be more important for gaining access and developing good working relationships in Ways and Means. The results obtained here suggest that this is the case.

Voting

Estimated coefficients for the voting models are reported in Tables 3 and 4. The OLS estimates for equation 3 are reported in Table 3; the 2SLS estimates for equation 4 are reported in Table 4.²¹ Results of the OLS estimation reveal the statistical significance of three of the four lobbying coefficients. Only lobbying by groups opposed to the substitute amendment by Representative English on the farm issue failed to attain statistical significance in the expected direction, although the coefficient does have the anticipated sign. One may wish to interpret these coefficients with caution, however, given the possible bias resulting from correlation with the error component of voting.

Importantly, none of the coefficients for campaign contributions exhibit statistically significant effects in the OLS estimation.²² The coefficients for contributors in the Ways and Means model are

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Table 3. OLS Estimates of Committee Voting

Variable	Ways and Means	Agriculture
Intercept	1.63 (1.28)	-.57 (-1.12)
Party (0 = D, 1 = R)	-.51 (-.57)	-.08 (-.24)
Ideology (ADA voting score)	-.01 (-.78)	.006 (1.01)
Constituency (grain/chemical production)	.07 (.31)	.001 (.67)
Contributors _f (number of groups contributing for)	-.30 (-.96)	-.14 (-.63)
Contributors _a (number of groups contributing against)	.30 (.77)	.01 (.01)
Money _f (total contributions for)	.37 (.99)	.47 (.77)
Money _a (total contributions against)	-.29 (-1.15)	-.04 (-.27)
Lobby _f (number of groups lobbying for)	.18** (2.70)	.44** (3.32)
Lobby _a (number of groups lobbying against)	-.21* (-1.30)	-.004 (-.82)
Number of observations	35	40
R-squared	.54	.51
Corrected R-squared	.37	.36

Note: t-ratios are given in parentheses.

* $p < .10$.

** $p < .05$.

fairly large (roughly as large as the coefficients for the lobbying variables) but their signs are in the wrong direction. The greater the number of contributors supporting the broad-based tax, the fewer the number of votes in support of the tax. The coefficients for total contributions, while pointing in the expected direction in each case, are not statistically significant. Even when tested jointly, the null hypothesis that total contributions and total number of contributors together are not equal to zero could not be rejected, given the relatively high collinearity between the contribution variables. Thus, even in the less visible committee setting, campaign contributions exhibited little effect on voting

decisions once organized lobbying efforts were taken into account.

The results of the 2SLS estimation (Table 4) indicate that committee voting decisions, particularly within the Ways and Means Committee, were significantly affected by organized lobbying efforts.²³ In fact, lobbying contacts exhibit the only statistically significant effects on voting in both committees. Voting did not divide sharply along partisan or ideological lines, for either issue nor were constituency characteristics statistically significant determinants of voting choices for either.²⁴ These results suggest that interest group lobbying, in contrast to much of the prevailing wisdom, can have a signifi-

Table 4. 2SLS Estimates of Committee Voting

Variable	Ways and Means	Agriculture
Intercept	1.22 (.61)	-1.30 (-.97)
Party (0 = D, 1 = R)	-.21 (-.19)	-.03 (-.08)
Ideology (ADA voting score)	.002 (.08)	.01 (1.08)
Constituency (grain/chemical production)	.14 (.57)	.001 (.33)
Lobby _f (number of groups lobbying for)	.24** (1.72)	.56* (1.60)
Lobby _a (number of groups lobbying against)	-.45* (-1.45)	.001 (.01)
Number of observations	35	40
R-squared	.46	.47
Corrected R-squared	.37	.39

Note: t-ratios are given in parentheses.

* $p < .10$.

** $p < .05$.

cant impact on representatives' voting choices.

Tests of statistical significance here are complicated by relatively high correlations among some of the independent variables. This multicollinearity may be partly responsible for the small t-values associated with partisanship and ideology, variables that typically emerge as powerful predictors in congressional voting models. Thus, for each possible pair of independent variables in each of the voting equations, I conducted an F-test on the null hypothesis that coefficients for both variables were zero. The null hypothesis was rejected in four instances. For the superfund issue, the coefficients for party and lobby_f, and ideology with each lobbying variable, were statistically different from zero. For the agricultural issue, the coefficients for party and lobby_f were jointly different from zero. These results suggest that party and ideology may be more important predictors of voting than the individual hypotheses tests indicate. At any rate, a larger sample is

needed to sort out these effects.

Within the Ways and Means Committee, lobbying from the two coalitions had significant but opposite effects. Representatives were more likely to support the broad-based tax, the greater the number of groups lobbying them in support of the tax. And the greater the number of groups lobbying them against the tax, the less likely they were to support the proposed tax. Thus, representatives tended to weigh the competing interests on both sides of the issue. The marginal effect of one additional group lobbying was greater for groups opposed to the broad-based tax, suggesting that a lobbying contact from an oil and chemical company loomed somewhat more important. This result is sensible in that organizations such as the American Petroleum Institute, Standard Oil, and Dow Chemical Company are more visible and prestigious than many of the organizations in the opposing coalition (e.g., the International Association of Ice Cream Manufacturers, National Pasta Association, etc.). Yet

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because the oil and chemical interests were outnumbered by groups supporting the broad-based tax, the general business interests were successful in getting a compromised version of the broad-based tax passed through the committee.

The effects of lobbying efforts in the Agriculture Committee are not nearly as striking. Lobbying by groups in favor of supply management and income guarantees in the form of higher target prices and loan rates were weakly significant in statistical terms, but lobbying by the opposing groups exhibited virtually no effect. In fact, voting in the Agriculture Committee did not divide significantly along any of the usual dimensions—party, ideology, constituency, or groups. The lack of any salient voting cue for this issue was clearly reflected in the difficulty the Wheat, Soybeans and Feed Grains Subcommittee had in reporting a bill to the full committee. Ultimately, in an unusual display of indecision, the subcommittee reported a “triple option” to the full committee; and the particulars of the commodity programs were hammered out in the full committee markup.

That lobbying efforts were less important in the Agriculture Committee than in Ways and Means may be a consequence of the fact that interest group access is less costly in Agriculture. When less of a premium is placed on access, committee members are more likely to hear from and to consider the preferences of groups on all sides of the issue. Thus, group appeals are less of a discriminating factor in representatives’ vote choices. Moreover, on important issues, especially the General Farm Bill of 1985 and the severe economic crisis in the agricultural economy, the Agriculture Committee behaves much like a policy committee (see, e.g., Smith and Deering 1984, 106). On such important matters committee members may rely more on their own expert judgments than on partisan, ideological, and group cues.

One possible problem with the esti-

mated influences of lobbying here is that the survey instrument did not specifically ask lobbyists to limit their responses to lobbying on just the committee proceedings. As it turned out, an important floor vote on the tax plan overturned the outcome from the Ways and Means Committee. An open rule allowed Representative Downey to reintroduce his amendment striking the broad-based tax, and this amendment passed by a 220–206 margin. Since groups lobbied extensively on this floor vote, the estimates of lobbying on committee voting may be biased by the groups’ efforts on the later vote, especially if groups concentrated their efforts on the representatives who supported them in committee. To investigate this possible source of bias, I also included the floor vote on the Downey Amendment in the analysis. This estimation yielded equally strong, if not stronger, effects of lobbying on voting choices. The lobbying influence of the coalition opposed to the Downey Amendment on the floor was reflected in the voting of the Ways and Means members: only 14 supported the Downey Amendment, and 22 opposed it. Thus, the chemical and petroleum groups did even better among committee members on the floor vote than they did within committee—even though they lost the floor fight.

Conclusions

Representatives’ voting decisions in committee, particularly in the Ways and Means Committee, are best explained here by the number of lobbying contacts they received from groups on each side of the issue. Campaign contributions proved to be useful predictors of groups’ lobbying patterns; but once lobbying was controlled, little evidence was found in support of a direct link between money and voting. Consistent with the popular notion that money “buys” access but not votes, campaign contributions influenced

voting decisions indirectly through lobbying.

Both party and ideology were found to be good predictors of lobbying. But legislators' policy predispositions—as reflected by party and ideology—are not the sole determinants of lobbying activity. The clear, but moderate, effect of campaign contributions on lobbying suggests that groups often structure their lobbying activities around prior campaign contributions. These contributions, once in place, may facilitate access and amplify lobbying messages. Groups that distribute their contributions broadly across the partisan and ideological spectrum gain flexibility in subsequent choices about whom to lobby. Instead of lobbying just their partisan and ideological friends, as the standard literature predicts, groups may be able to reach out effectively to a broader and more diverse audience through campaign contributions.

That party and ideology were important for predicting groups' lobbying patterns suggests that the lobbying patterns representatives observe are generally consistent with both their own general policy outlooks and those of their financial contributors. In this sense, organized lobbying functions much as the conventional wisdom suggests—it reinforces. But the evidence here also suggests that lobbying exerts an important *independent* effect on representatives' decisions. Representatives, therefore, are not simply captives of partisan and ideological forces. They respond and adjust to the balance of group pressures they experience. This evidence fits with the reality of the U.S. political system, where relatively heterogeneous, undisciplined, and nonideological parties leave room for substantial group influence (Eldersveld 1982; Schattschneider 1942). The evidence is also consistent with the reality that groups find it in their interest to spend millions of dollars lobbying members of Congress each year. Finally, the evidence is consistent with aspects

of both the "pressure groups" model of interest group influence (representatives respond to competing group forces, see e.g., Latham 1952) and the "communications perspective" (lobbying reinforces representatives' partisan and ideological predispositions; see, e.g., Bauer, Pool, and Dexter 1963).

Campaign contributions—much like partisan and ideological factors—also affect voting decisions indirectly through lobbying. Whether this relationship might be stronger for different committees or different issues and whether it might be stronger for some types of groups and legislators than others are questions that deserve further scrutiny. Most important, though, is the evidence that money enters the process other than through a direct exchange of contributions for favors. Since lobbying is significantly related to previous contributing, the political and technical information representatives receive from groups is shaped to some extent by campaign money. This result helps explain the importance that groups attach to campaign contributions in light of their claims that they cannot "buy" votes with PAC money and in light of the small empirical effects of money on voting that scholars frequently observe. This may be small consolation to those concerned about the pernicious effects of money on policy making, but it underscores the fact that representatives' votes cannot be purchased outright.

Certainly, much can be learned by introducing measures of organized lobbying efforts into congressional voting models. Additional refinement is needed, however, and a clearer understanding of how representatives process the vast information that comes their way through lobbying contacts must be developed. It appears, as well, that lobbying is more important in some committees and on some issues than others. These differences also deserve further exploration. Hopefully, empirical evidence that lobbying is impor-

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tant to the congressional policy process will encourage further theoretical work on groups' lobbying strategies. These theoretical results are clearly needed to guide future data collection and the specification of statistical models.

Appendix

Question Wording

Lobbyists were asked to indicate whether or not their organization engaged in any of 11 different lobbying activities for the particular bill in question and to indicate the percentage of the organization's *total* lobbying effort devoted to that activity. The 11 activities were congressional testimony, publication of a policy paper, inclusion of the organization's name with other groups in a letter addressed to representatives, organizing a letter-writing campaign among group members, organizing a telephone campaign for calls to members' district offices or Washington offices, purchase of advertising in newspapers or other media, arrangement of district-level meetings between representatives and group members, arrangement of meetings in Washington between representatives and group members, personal contacts between representatives and their staffs and the organization's professional lobbyists, and representation through a professional lobbying or law firm in Washington.

After responding to these items, lobbyists were asked the following question: "Of the lobbying activities in this list that were used by your organization, roughly how much effort did your organization expend overall on these activities to enlist or to maintain the support of the following members of the House [Agriculture or Ways and Means] Committee? (*A great deal, some, little or none.*)

Notes

An earlier version of this paper was presented at the annual meeting of the American Political Science Association, Washington, 1988. I thank Janet Grenzke, John Green, and Rick Hall for their comments on various drafts of this paper. This research has been supported by National Science Foundation Grant SES 85-20809.

1. Figures are reported in "Foes of Acid-Rain Bill Led '86 Lobbyist Spending," *New York Times*, June 1987, p. 13.

2. Under the existing commodity programs, farmers who received commodity loans from the federal government were allowed either to pay back the loan in cash or to default on the loan and turn their crops over to the government. These loans were designed to allow cash-short farmers to spread out their grain sales and avoid having to dump their grain on glutted markets. Naturally, when market prices exceeded the established loan rate, farmers sold their grain on the market and paid back the loan in cash. But when market prices dipped below the loan rate, farmers defaulted on their loans and the grain was placed in federal reserve. During the 1970s, market prices typically exceeded loan rates, so that farmers seldom defaulted on their loans. But as commodity prices fell from 1981 to 1984, market prices began to dip below the loan rate, causing more and more farmers to default on their loans.

3. Salisbury and his colleagues (1987) employed some of these same approaches in identifying organizations relevant to particular policy areas. But due to their interest in coalitional patterns in general policy areas rather than specific bills, their approach differed somewhat from the approach employed here.

4. Moreover, missing data precluded the use of other questions in my analysis here. In particular, a question dealing with the organizational strength of groups in congressional districts was missing for many organizations that reported lobbying information. Dropping these groups from the analysis substantially distorted the measures of coalitional lobbying.

5. The following organizations reported lobbying activity: American Petroleum Institute, American Standard, Apache Corporation, Association of General Merchandise Chains, Atlantic Richfield, Cabot Corporation, Chevron USA, Congoleum Corporation, Consolidation Coal Company, Danielle Battery Manufacturing Company, Dow Chemical Company, General Foods Corporation, Hazardous Waste Treatment Council, International Association of Ice Cream Manufacturers, Kerr McGee Corporation, Manufactured Housing Institute, Mapco, Mining and Reclamation Council of America, Mississippi Asphalt Pavement Association, National Association of Chemical Distributors,

National Association of Convenience Foodstores, National LP Gas Association, National Lumber and Building Materials Association, National Pasta Association, National Petroleum Refiners Association, National Soft Drink Association, National Wildlife Federation, Occidental International Corporation, Phillips Petroleum Company, Quaker Oats Company, Standard Oil Company, St. Joe Minerals Corporation, Sun Company, Voltmaster, Waste Management.

6. In addition, the organizations that led the coalition against the broad-based tax were noticeably less cooperative in the personal interviews than the leading groups in support of the broad-based plan. This general attitude may have affected response rates among other groups in the coalition.

7. Groups included in the analysis are the Texas Corn Growers Association, Union Pacific Railroad, Maryland Farm Bureau, American Bakers Association, Texas Farm Bureau, American Agricultural Movement, National Farmers Union, National Farmers Organization, National Cattlemen's Association, American Meat Institute, Burlington Northern Railroad, American Feed Manufacturers Association, Minnesota Farm Bureau, Michigan Farm Bureau, National Barley Growers Association, United Aerospace and Agricultural Implement Workers Union, National Grange, Cargill, National Grain and Feed Association, National Corn Growers Association, American Farm Bureau Federation, American Soybean Association, Georgia Farm Bureau, Louisiana Farm Bureau, Iowa Farm Bureau, Continental Grain Company, Bread for the World, Minnesota Corn Growers Association, and the Oil, Chemical, and Atomic Workers Union. There was also one organization that removed its identification tag.

8. I assume the contribution variables are uncorrelated with the errors, u and v , in lobbying. In making this assumption, I assume that groups' contribution decisions in the preceding election cycle are constrained largely by electoral considerations, not subsequent lobbying considerations. Thus, a group's contributions in the preceding election cycle are not made necessarily in anticipation of lobbying in the following legislative session; but once the contribution has been made, it may help the group secure access and thereby influence its decisions to lobby. This assumption would be harder to justify, of course, for contributions given during the legislative session in which the lobbying took place.

9. I estimated an alternative specification in order to test this assumption and found only weak relationships between the two lobbying variables. While I found instruments to estimate the simultaneous relationship between the lobbying variables, I was not at the same time able to satisfy the rank condition when estimating the voting equation.

10. As with the lobbying equations, I assume that the voting equations are linear functions, and that e_1

and e_2 (equation 4) are error components.

11. The direction of bias is hard to predict. Representatives may reveal private information that is either favorable or unfavorable from a group's standpoint, and the group may react by lobbying more or by lobbying less.

12. An alternative specification is to assume that party, ideology, and constituency considerations affect the lobbying decision *indirectly* through some *estimate* of the expected vote. Thus, one could compute an expected vote as a function of partisanship, voting scores, and the like, and use this estimate, together with contributing and leadership, to predict lobbying. I estimated the model in this form but found the "expected vote" to have little (i.e., no statistically significant) effect on lobbying. For the issues selected here party, ideology, and constituency were not very reliable guides to voting. Unfortunately, there is little theoretical or empirical evidence to suggest how groups anticipate who their potential friends and foes are when it comes to lobbying.

13. Of the 43 members of Agriculture four were first elected in 1984. ADA scores for these members were based on votes during just the first session of the 99th Congress.

14. These data were taken from the U.S. Census of Agriculture and the U.S. Census of Manufacturers, Geographic Series for 1980, which reported data at the county level. These county figures were aggregated into the corresponding congressional districts. Whenever a county was split across congressional districts, the county was included in the district if at least one-half of the land area of the county was included within the district.

15. I am grateful to one Washington-based organization for sharing their private notes of this markup session.

16. See original transcripts (U.S. Congress, House Committee on Agriculture) and *Minutes of the Business Meetings and Hearings*, 99th Cong., 1st sess.

17. Results of the analysis for voting on the Foley Amendment were very similar to the results on the English Substitute.

18. Later, on the House floor, many conservatives opposed the broad-based tax, fearing that it was a value-added tax.

19. Although one might think that districts of the Agriculture Committee would not vary much with regard to grain production, this is not the case. Production varies from just under one million bushels to more than 500 million bushels. The mean production is 106 million bushels, with a standard deviation of 127 million.

20. The relatively stable configuration of interests in agricultural policy has been expanding somewhat in recent years, though (Browne 1986).

21. In small samples, 2SLS estimates will be inconsistent. Thus, one statistical reason for estimating

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the voting model with OLS (equation 3) is the relatively small size of the samples under analysis.

22. In addition to the OLS estimation, I estimated representatives' voting decisions on each of the amendments separately in a probit analysis, employing the same explanatory variables as in equation 3. Of the 20 contribution coefficients (4 variables across 5 votes), only 4 proved statistically significant and in the correct direction. In contrast, 9 of the 10 coefficients for lobbying (2 variables across 5 votes) were significant at least at the .1 level of significance.

23. The Ns for Tables 3 and 4 differ slightly from those in Tables 1 and 2 because several representatives did not cast a vote on each of the issues. I reestimated the coefficients for the lobbying equations excluding the representatives who missed votes. Because the differences between the two estimations were so slight, I reported the estimates from the larger sample in Tables 1 and 2.

24. I also examined the effects of representatives' electoral margins in 1984, expecting that groups might be likely to lobby representatives from relatively safe districts. This variable did not prove to be statistically significant.

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THE EXPECTED UTILITY THEORY OF CONFLICT: MEASURING THEORETICAL PROGRESS

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We examine the "progressivity" of Bruce Bueno de Mesquita's theory of conflict as originally developed in *The War Trap* and as extended later. Bueno de Mesquita offers the progressivity of the expected utility theory relative to other theories and approaches to conflict as his major defense in responding to critics. Bueno de Mesquita essentially relies on Imre Lakatos' definition of theoretical progress in presenting his argument. A review of the literature addressing the concept of theoretical progress indicates that Bueno de Mesquita's application of Lakatosian criteria is incomplete and that Lakatos' criteria are themselves flawed. We review the philosophy of science literature dealing with theoretical progress or rational criteria for theory choice and evaluate the progressivity of the expected utility theory of conflict in light of criteria other than Lakatos'. While we do recommend further elaboration of Bueno de Mesquita's theory, we do not find it more progressive than its rivals.

Approximately 15 years ago major scholars in international politics met at Santa Monica, California to discuss the state of the discipline.¹ One of their major concerns was the general lack of progress in theorizing about international politics and the noncumulative nature of research in their field. A consensus emerged from this conference concerning what was required in order to achieve theoretical progress. Conference participants agreed that the development of theories and the testing of theoretically predicted results were necessary steps for promoting greater progress and understanding about the matters of concern to them. This approach has had a major impact on the study of international politics. Since 1973 a small, but significant

number of researchers have attempted to develop axiomatic, deductive theories of international phenomena. Bruce Bueno de Mesquita's analysis of conflict has been the most exciting and ambitious effort in this direction, judging from the sheer volume of responses it has generated in the major professional journals. The publication of Bueno de Mesquita's expected utility theory of conflict originally appeared in the *American Political Science Review* in 1980 and was subsequently published in more extensive form the following year in *The War Trap*. *The War Trap* soon underwent several extensions (Bueno de Mesquita 1985a; Bueno de Mesquita and Lalman 1986; Morrow 1985, 1986) and subsequently became the object of an extended debate in the international rela-

tions literature (Bueno de Mesquita 1984a, 1984b, 1987; Khong 1984; Majeski and Sylvan 1984; Nicholson 1987; Wagner 1984).

Our primary purpose is to assess what has become of Bueno de Mesquita's principal defense in the debate over his expected utility theory. Throughout his replies to critics, Bueno de Mesquita offers the progressiveness of his theory as its central defense; that is, he argues that the expected utility theory, while not immune to all problems, is nonetheless superior to competing theories because it is more progressive than any alternative theory in the field. By evaluating the progressiveness of his theory, we will be able to clarify and resolve many points of disagreement in the exchanges between Bueno de Mesquita and his critics. Because this type of debate plays an important role in the advancement and growth of knowledge, we believe that our efforts to clarify and resolve many of the issues raised in this debate will promote the refinement of existing theories and the advancement of better ones. Finally, because Bueno de Mesquita's theory is the best example of the kind of axiomatic deductive approach advocated by many scholars in international politics (Bueno de Mesquita 1985b; Jervis 1985; Krasner 1985; Morrow 1986) our analysis will inevitably shed light on the progressiveness of this broader deductive approach.

First, we review and appraise Bueno de Mesquita's arguments concerning the progressive qualities of his theory. After identifying several difficulties with his evaluation, we discuss alternate criteria developed by philosophers of science to assess theoretical progress. We then proceed to analyze the strengths and weaknesses of each side in the debate over Bueno de Mesquita's theory. Finally, we conclude with an overall appraisal of Bueno de Mesquita's theory and what it has (or has not) contributed to progress in our understanding of war.

Appraising the Progressiveness of Bueno de Mesquita's Theory

Bueno de Mesquita's appraisal of his own theory is based primarily upon his interpretation of Imre Lakatos. According to Lakatos, "A scientific theory T is falsified if and only if another theory T' has been proposed with the following characteristics: (1) T' has excess empirical content over T : that is, it predicts novel facts; that is, facts improbable in the light of, or even forbidden by, T ; (2) T' explains the previous success of T , that is, all the unrefuted content of T is included (within the limits of observational error) in the content of T' ; and (3) some of the excess content of T' is corroborated" (1978, 32). With these criteria in mind, Bueno de Mesquita argues that his expected utility theory of conflict is T' ; that is, he argues that his theory not only explains many problems that have been of central concern to students of international conflict, but it (1) does so within a single integrated theoretical framework, (2) explains or accounts for novel (or additional) facts not detected in other theories, and (3) possesses greater degrees of corroboration (1984a). He demonstrates all of the above by comparing expected utility hypotheses drawn from his own and others' studies (Altfeld and Bueno de Mesquita 1979; Berkowitz 1983; Newman 1982) with competing hypotheses addressing the same phenomena. In *The War Trap* (1981), for example, Bueno de Mesquita compares Kissinger's hypothesis linking balance of power to war initiation with his own derived hypothesis linking expected utility to war initiation. He finds the latter a better predictor of war initiation, relying on a proportionate reduction in error methodology. Likewise, in an article written with Altfeld (Altfeld and Bueno de Mesquita 1979), Bueno de Mesquita compares his expected utility theory with a study by Randolph Siverson and J. King predicting whether or not third par-

ties will participate in ongoing wars. Relying again on the proportionate reduction in error methodology, Bueno de Mesquita finds expected utility a better predictor of the dependent variable. He noted as well that Siverson and King's approach predicts only whether a third party will participate in an ongoing war or not. His study on the other hand predicts not only participation but also on which side nations will fight. Bueno de Mesquita thus concludes that his own study "satisfies Lakatos' criteria, at least in comparison with Siverson and King's study of the same problem" (1984a, 345). Using the same head-to-head comparison approach, he concludes that in general his theory is more progressive than any other theory in the field.

Our major argument here is that even if one accepts the criteria used by Bueno de Mesquita, a closer scrutiny of Lakatos' views on this issue indicates that Bueno de Mesquita has decided prematurely on the progressivity of his own theory. In his theory of scientific rationality, Lakatos argues that decisions of acceptance and rejection are made between competing "research programmes" rather than between competing theories. Every research programme is composed of a series of individual theories, each of which accounts for all the empirical content of predecessor theories in the series. In addition to a series of minitheories, research programmes also contain negative and positive heuristics. The negative heuristic, or *hard core*, of a research programme identifies the assumptions that are irrefutable and cannot be altered when theoretically predicted outcomes are refuted by experimental outcomes. The positive heuristic, on the other hand, consists of the assumptions that can be modified when refutations arise between predicted and observed outcomes. In progressive research programmes, modifications of the programme's positive heuristic produce a succession of theories that account for all the

empirical content of predecessor theories while also predicting unknown or novel phenomena. A degenerating programme, on the other hand, is one in which repeated attempts to modify the positive heuristic fail to predict any novel phenomena not already predicted by predecessor theories in the series. For Lakatos, theoretical progress is made when progressive research programmes are accepted and their degenerating rivals are rejected.

Using Lakatos' work as a backdrop, Bueno de Mesquita's appraisal of his own theory seems to imply that the theories to which he compares his expected utility theory comprise a series of theories within a single research programme. His is merely the latest and most progressive theory in this series. To argue this, however, Bueno de Mesquita should provide us with some criteria for identifying this research programme so that we know a priori the theories it encompasses and therefore the sum total of theories and hypotheses to which his expected utility theory should be compared. By failing to provide such defining criteria, Bueno de Mesquita gives the (quite possibly mistaken) impression that he chooses to compare his theory only to those that make it look better. Just as importantly, Bueno de Mesquita's failure to identify all theories in the research programme makes it impossible to see if his theory actually entails all the undisputed content of its predecessors. Until Bueno de Mesquita identifies the research programme and the entire series of theories in it, he has not fulfilled Lakatos' criteria for demonstrating that his theory is superior to others in the series.

It is possible, of course, that Bueno de Mesquita does not regard his theory of conflict and the theories referred to in his comparisons as belonging to the same research programme. He may regard them as belonging to rival research programmes. We can infer this position from

his reference, on occasion, to the empirical successes of "the expected utility approach" (1984a, 1984b, 1987). Because the explanations or hypotheses used in his comparisons do not adopt an expected utility approach, he might believe that they belong to rival research programmes. But in order to determine if his research programme is superior to its rivals, Bueno de Mesquita must first identify these rival programmes and then show that his programme is progressing while rival programmes are degenerating; that is, he needs to show the degenerative nature of their positive heuristics and the progressive nature of his own. Until he does this, Bueno de Mesquita cannot claim, at least by Lakatosian standards, that his expected utility approach (i.e., research programme) is more progressive in that it "provides a better understanding of international conflict than any widely tested alternative" (1984a, 342). Such a claim requires, at least according to Lakatos, a much more comprehensive comparison of rival research programmes than Bueno de Mesquita has provided.

Even if Bueno de Mesquita attempts such a task, however, he is unlikely to succeed. This is because Lakatos' criteria themselves are flawed. According to Lakatos, the identification of a research programme and its constituent theories requires an identification of its hard core. Yet in efforts to identify the hard cores of research programmes in the natural sciences, several philosophers of science have found that they are not as hard and identifiable as Lakatos believes (Berkson 1974, 1976; Laudan 1977; Musgrave 1976). Hard cores are not immutable and have been shown to evolve over time. Since a programme's constituent theories all share the same hard core, several philosophers of science have argued that it would be difficult if not impossible to determine the series of individual theories that comprise a particular research programme. Additionally, both Kuhn (1962)

and Feyerabend (1978) have demonstrated that at least in the natural sciences, there are knowledge losses as well as knowledge gains in the acceptance of any new theory within a single research programme; that is, the series of theories in a single research programme is not nearly as cumulative as Lakatos maintains. If there are losses as well as gains in the empirical content of successive theories within a single research programme in the social sciences as well, Lakatos' criteria would be no use in identifying "progressive" successor theories within the programme. For the same reason, Lakatosian standards would be no help in determining which programmes were progressive and which ones were degenerating. Moreover, Lakatos' approach implies that we should immediately reject degenerating programmes and accept their more progressive rivals. But according to many philosophers of science, this would be a mistake. There is no guarantee that a currently degenerating research programme will not become progressive at a later time. And even setting a specified time limit in which to judge the degenerative or progressive nature of a research programme, as some analysts have suggested, would be both arbitrary (Musgrave 1976) and counterproductive to the actual progress of science (Feyerabend 1970, 215; Kuhn 1970; Nicholson 1983). This is because a decision to reject a degenerating research programme at some specified point would undermine further efforts to account for apparent theoretical anomalies, a practice that has often generated major theoretical advances in the past.

If Lakatos' criteria for assessing theoretical progress are inappropriate, where do we turn to find such criteria? Before Lakatos, philosophers of science argued that confirmability and falsifiability were appropriate measures of theoretical progress. A theory's degree of confirmability depends on the extent to which observed

instantiations of the theory conform to theoretical predictions. The greater the correspondence between theoretical predictions and observed instantiations of the theory, the greater the degree of confirmability. Progress is achieved when theories with greater degrees of confirmability are accepted and less-confirmed rivals are rejected. Critics of this approach pointed out, however, that even if all observed instantiations of a theory conformed to its predictions, no number of observed instantiations could confirm a theory. This is because theories contain lawlike statements; and laws, by definition, apply to an infinite number of observational statements. Thus no finite number of observations could confirm an open-ended theoretical assumption (Moon 1975; Nicholson 1983). Recognizing this problem, Popper (1963) subsequently proposed falsifiability as the criterion for ascertaining theoretical progress. Although no finite number of observations could completely confirm a theory, falsifying a theory required only one counterexample. Progress is made when falsified theories are rejected and unrefuted theories are accepted. Critics of this approach have noted however, that it is always possible to save a theory from refutation by arguing that the data are false, that the indicators are crude, or that the measuring instruments are imperfect. They point out that when a substantive theory is tested, theories about measuring instruments and testing procedures are also inevitably involved. As a result, when an experimental outcome is inconsistent with the theoretically predicted results, it is impossible to know exactly which theory is responsible for the refutation (Agassi 1964; Duhem 1954; Laudan 1977; Quine 1953).

If Lakatos' approach to assessing theoretical progress is fundamentally flawed and if neither confirmability nor falsifiability are workable measures of the same, is the search for a rational measure of

theoretical progress unrealistic, as Kuhn (1962) and Feyerabend (1978) suggest? We think not; and we offer as the best available alternative the approach of Lawrence Laudan. Agreeing with Kuhn and Feyerabend that the evolution of scientific theories is noncumulative, Laudan (1977) argues that this does not imply that there is no continuity in the evolution of these theories. Laudan sees the facts in need of explanation or the empirical problems scientists wish to solve as the one major element of continuity among scientific theories within the same domain of inquiry. He points out for example, that "since the 1640's every optical theory has had to deal with what happened to light when it was refracted through a prism. Since about 1800, every theory of chemistry has had to deal with the role of air in the process of combustion. Since the 1650's every theory of matter and gaseous states has been forced to explain the (approximately) inverse relations between pressure and volume of gases" (p. 140).

For Laudan, the most progressive theory choices are the ones that "maximize the scope of solved empirical problems, while at the same time, minimizing the scope of unsolved anomalous or conceptual problems" (1977, 66). Empirical problems are the substantive problems that scientists in any discipline feel are in need of explanation. Questions like "Why do some disputes lead to violence while others are settled without resort to violence?" or "Do nations who join alliances enhance or jeopardize their security?" are examples of such empirical problems in international conflict. Anomalous problems, a subcategory of empirical problems, arise when observations are inconsistent with theoretical predictions. A theory solves empirical or anomalous problems by logically implying an approximate statement of the problem. Conceptual problems are of two types: internal and external. An internal conceptual problem arises when a theory exhibits

either an internal inconsistency or contains vague and unclear concepts. Internal conceptual problems are eliminated when concepts are clarified or an internal inconsistency is removed. External conceptual problems may arise when scientists believe that a specific type of relationship should exist between two theories but the relationship does not obtain. Scientists may feel, for example, that two theories should be compatible; but they turn out not to be. Or scientists may feel that one theory should imply the other, but it fails to. If any of these situations arises, an external conceptual problem exists for the theory under investigation. In short, "any relation short of entailment can be regarded as posing a conceptual problem" (1977, 54). External conceptual problems may also occur when a theory is incompatible with its methodology; that is, there are questions concerning the appropriateness of testing procedures, the data used in the tests, the interpretation of the data analyses, and the like. External conceptual problems are eliminated when the methodology is made compatible with the theory under investigation or the theory under investigation bears the expected relationship to another relevant theory.

This discussion points to some important differences between Lakatos' and Laudan's criteria for theoretical progress. First, Lakatos requires a theory in a series to entail all the undisputed empirical content of its predecessors for the theory to be progressive. Laudan, on the other hand, does not make progress contingent on such cumulation. For Laudan, a theory may be progressive even if it fails to account for all the undisputed empirical content of its predecessors. As a result, Laudan's criteria for progress do not require us to assess cumulation among the series of theories within a single research programme. Secondly, according to Lakatos, to appraise progress, we need only to determine whether or not successive modifications have produced an in-

crease in the number of solved empirical problems; that is, progress is achieved when modifications of a programme's positive heuristic produce a progression of content-increasing theories. The conceptual problems that may be generated by these modifications are not relevant to Lakatos' criteria for ascertaining progress. On the other hand, Laudan's criteria for evaluating theoretical progress require us to examine a reduction in the number of conceptual problems as well as an increase in the number of solved empirical problems. As Laudan points out, conceptual problems have figured prominently in major criticisms leveled against scientific theories. He notes for example, that "many scientists criticized Ptolemy's epicyclic astronomy not because it was unable to solve significant empirical problems (because it could), but rather because of its use of equants and eccentrics as well as epicycles." Likewise, "Newton's critics were concerned about the conceptual ambiguities and foundational assumptions of his theory. What was absolute space and how could bodies act on one another at a distance?" (1977, 46). Similarly, many of the criticisms leveled against Bueno de Mesquita's work are conceptual. Therefore, if we are to understand both the relevance and significance of many of these criticisms, we need to rely on a theory of progress that gives a central role to conceptual problems. Furthermore, if we neglect to include conceptual problems in our appraisal of theoretical progress, we are unable to comprehend several subsequent revisions and modifications of the model originally developed in *The War Trap*. These considerations lead us to believe that Lawrence Laudan's criteria best allow us to assess Bueno de Mesquita's theory of conflict and the debate it has generated. We turn now to a discussion of the debate surrounding Bueno de Mesquita's theory of conflict in terms of Laudan's criteria for progress.

Solved and Unsolved Problems in Bueno de Mesquita's Theory of Conflict

Conceptual Problems

Internal Conceptual Problems. There are two major kinds of internal conceptual problems that plague Bueno de Mesquita's expected utility theory. The first of these—use of vague and ambiguous concepts—has already been discussed in relation to his work by other authors. Majeski and Sylvan (1984), for example, emphasize the "reductionism" and ambiguity of such concepts as *utility*, *national foreign policy*, *possible opponents of potential war initiators* and the like. They argue that these concepts, as used by Bueno de Mesquita in *The War Trap* are ambiguous and that the scope of their meanings are reduced in actual use. Luterbacher (1984) argues that by only taking into account what third parties might or might not do, Bueno de Mesquita's conceptualization of risk taking is too restrictive and does not capture most of what we mean by this concept. We do note Laudan's observation that many theories often begin with vague and ambiguous concepts that work to their advantage in that they allow the theorist to extend the theoretical concepts to their broadest applicable domain (1977, 49), which suggests that some attacks on Bueno de Mesquita's work may be informed by a too-fervent devotion to the religion of science in principle rather than science in practice.

We focus our attention instead on a second, largely unnoted internal conceptual problem of Bueno de Mesquita's theory—the logical validity of arguments linking the theory's assumptions to its derivations. In any deductive theory, the conclusions or the problems to be explained need to be logically entailed by the theory's assumptions. Anything short of logical entailment between the assumptions and reputed conclusions is an inter-

nal conceptual problem.² Bueno de Mesquita's models in *The War Trap* and "The War Trap Revisited" (1985a) contain several of these problems. To illustrate these conceptual problems, we need to review the basic premises and conclusions of Bueno de Mesquita's theory. We include the assumptions and conclusions only of *The War Trap* and "The War Trap Revisited" because the related works of Bueno de Mesquita and others (e.g., Berkowitz 1983; Bueno de Mesquita 1983) are not part of the deductive framework of his theory. The latter studies are more properly additional assumptions or hypotheses stated in the language of expected utility but not playing a critical role in most of the theoretical derivations.³

By describing conflict in expected utility terms, Bueno de Mesquita makes several basic assumptions of classical expected utility analysis. He assumes, for example, that preference orderings are transitive and that preference orderings can be represented by utility functions. To these, Bueno de Mesquita adds the assumptions necessary for his theory. Specifically, he assumes that governments are unitary decision makers who behave as if they are expected utility maximizers. Then Bueno de Mesquita makes the most fundamental assumption of his theory, enabling him to derive a variety of propositions. He assumes that a positive expected utility is a necessary but not sufficient condition for war initiation.⁴ Specifically, he assumes that when the overall expected utility of a potential war initiator i is less than zero, it is not rational for i to initiate a war against nation j ; that is, when $E^i < 0$ (where E^i is the expected utility of initiator i), it is irrational for i to initiate war against its opponent j . Likewise, when $E^i \geq 0$, it is rational for i to initiate war against j . Note that because $E^i \geq 0$ is a necessary but not sufficient condition for war initiation when $E^i \geq 0$, we cannot conclude that i initiates war against j . Because we

find this statement to be one of the most critical assumptions in Bueno de Mesquita's theory we represent it as his major premise.

MAJOR PREMISE: *If war* $\rightarrow E^i \geq 0$.

This premise says that if war breaks out, the initiator must have had a positive expected utility. From this premise we can derive by the *modus tollens* operation that if $E^i < 0$, then nation *i* will not initiate a war. Thus, Bueno de Mesquita says that his theory, by assuming a necessary condition for war, enables him to infer sufficient conditions for peace. Several other assumptions are made about national risk taking under conditions of uncertainty, but their description is not necessary for our purposes. We turn now to the derivations listed in *The War Trap* (1981) and "The War Trap Revisited" (1985a).

"The War Trap Revisited." The first derivation we examine presents an example of an important internal conceptual problem. His (1985a) proposition 2 says that if $E^i \geq 0$ and $E^j \geq 0$, the probability of war approaches unity.

PROPOSITION 1. *If $E^i \geq 0$ and $E^j \geq 0 \rightarrow$ ($p[\text{war}]$ approaches 1), where E^j is the victim's expected value for war.*

This proposition, however, is not a valid inference of Bueno de Mesquita's theory. His major premise stipulates necessary conditions for war while our Proposition 1 describes sufficient conditions for war. Inferring our Proposition 1 from the major premise is in fact commonly referred to as the fallacy of affirming the consequent. His (1985a) propositions 3, 4 and 6 also specify probabilities for war when $E^i \geq 0$ and are invalid for the same reason. Bueno de Mesquita's proposition 3 says that when $E^i \geq 0$ and $E^j < 0$ and *j*'s perceived losses are less than *i*'s perceived gains, the probability of war is smaller for these situations than in

situations described by his proposition 2 (where both $E^i \geq 0$ and $E^j \geq 0$). His proposition 4 says that when $E^i \geq 0$, $E^j < 0$, and *j*'s perceived losses are greater than *i*'s perceived gains, the probability of war is smaller than in situations described by his proposition 3 (where *j*'s perceived losses are less than *i*'s perceived gains). Finally, his proposition 6 states that if $E^i \geq 0$ and *i* expects *j* to concede but *j* does not concede, there is a higher probability for violence than in situations where $E^i \geq 0$ and *i* expects *j* to fight but *j* perceives that it should instead concede to *i*.

The War Trap. Another example of invalid reasoning is his (1981) proposition 8. Bueno de Mesquita's proposition 8 says that when only one side in a war has positive expected utility, that side will be both the initiator and the winner. It also says that if both sides have positive expected utilities, the side with the larger utility will generally win. This statement, like his (1985a) propositions 2, 3, 4, and 6, is not a valid deduction because it stipulates a sufficient condition for war when the major premise provides only a necessary condition for war. Moreover, expected utility theory implies that the winner will be the nation with the higher probability of success, not necessarily the nation with the higher expected utility value (Nicholson 1987). The probability of success is the only factor involved in the calculation of expected utility values that may imply who the winner will be.

A different problem plagues his (1981) propositions 1, 3, 3', and 3''. These propositions are valid inferences; but when the derivations are restated as hypotheses, they are not logically equivalent to the original derivations. His proposition 1 says that nonaligned nations cannot rationally attack more powerful nations. Bueno de Mesquita derives this proposition by demonstrating that the expected utility of the weaker initiator will be negative, and therefore concludes that a nonaligned, less powerful nation *i* will not

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rationally initiate war.

PROPOSITION 2. *If i and j are nonaligned and $P_i > P_j$ then $(E^i < 0 \rightarrow \text{no war})$, where P_j is the power of the victim and P_i is the power of the initiator.*

This is a valid inference. In testing his proposition 1, however, Bueno de Mesquita restates it as, Conflicts among non-aligned states are initiated by the stronger adversary. This restatement of his proposition 1 is of the following form:

PROPOSITION 3. *If i and j are nonaligned and $P_i > P_j$ then $(E^i \geq 0 \rightarrow \text{war})$.*

Although our Proposition 2 is consistent with our Proposition 3, it is not logically equivalent to it. Our Proposition 2 logically follows from the assumptions of the theory while our Proposition 3 does not. In subjecting his theory to empirical tests, Bueno de Mesquita unfortunately tests the invalid inference of our Proposition 3.⁵

Another example of this same problem is seen in his propositions 3, 3', and 3''. These propositions specify conditions under which wars occur between allies. As was the case for his proposition 1, the problem of logical validity arises when the derivation is restated as a hypothesis. The derivations say that when certain conditions (x, y, z) obtain, nation i may initiate a war against its (former) ally, nation j . The critical word here is *may*. His propositions 3, 3', and 3'' are valid because they are tautologous; that is, the word *may* transforms each of these propositions into a form in which the consequent outcome (war) may or may not occur.

PROPOSITION 4. *If i and j are allies satisfying conditions x, y , and $z \rightarrow (E^i \geq 0 \rightarrow \text{war or no war})$.*

While these propositions are logically valid, Bueno de Mesquita restates them as invalid inferences when he subjects them

to empirical tests. In noting that "lovers' quarrels are far more common than most other types of conflicts" (1981, 160), he tests the proposition that "alliances increase a nation's chances of being attacked by its friends." The form of his propositions 3, 3', and 3'' represented by our Proposition 4 is now restated in his subsequent tests:

PROPOSITION 5. *If i and j are allies satisfying conditions x, y , and z then $(E^i \geq 0 \rightarrow \text{an increased probability of war between } i \text{ and } j)$.*

Our propositions 4 and 5 are not logically equivalent. While our Proposition 4 is a valid inference, our Proposition 5 is not. Once again, Bueno de Mesquita tests the invalid inference of our Proposition 5 in his analysis.

A third and somewhat related problem appears in his (1981) proposition 2. Here the proposition and restated hypothesis are valid inferences but they are both tautologous statements. His proposition 2 specifies conditions under which a non-aligned state can rationally attack a more powerful adversary. This proposition is valid because it is stated as a tautology, just as were his propositions 3, 3', and 3'' above. His proposition 2 says that if nation j is aligned and more powerful than nation i and if certain conditions (x, y) exist, i may attack j .

PROPOSITION 6. *If i is nonaligned, j is more powerful than i and has allies, and conditions x, y are true then $(E^i \geq 0 \rightarrow \text{war or no war})$.*

His proposition 2 is tested just as it is stated in our Proposition 6; but because it is tautologous, it is irrefutable and therefore does not tell us anything. That is, our Proposition 6 is true regardless of the antecedent conditions identified by Bueno de Mesquita.

A fourth problem involves his (1981) propositions 6 and 9. Here it appears that

the reasoning used to derive his proposition 6 is inconsistent with the reasoning used to derive his proposition 9. His proposition 6 says that if i 's expected utility from war with j is positive (i.e., $E^i \geq 0$ against j) but its absolute gain is less than j 's expected loss, (i.e., $|E^i| < |E^j|$), then both i and j are better off negotiating than fighting. This proposition can be restated.

PROPOSITION 7. *If $(E^i \geq 0$ and $|E^i| < |E^j|) \rightarrow$ no war.*⁶

The only condition under which Bueno de Mesquita can validly infer the absence of war is by showing that i 's positive expected utility changes to negative expected utility. His argument about how this change occurs depends upon the rationality of j . Because he assumes that j is rational, j should prefer situations involving smaller losses. Consequently, j concedes to i because j expects to lose more to i in a war than i expects to win. When this happens, $E^i < 0$ and it is not rational for i to initiate war.

His proposition 9 says that the greater the difference between what the initiator believes it can gain and what the opponent believes it must lose, the more difficult it is to find a settlement that both sides can accept, hence the costlier the war.

PROPOSITION 8. *The larger $|E^i - E^j|$, the more difficult it is to find a settlement and the more costly the war.*

Although this proposition addresses a situation in which war has already begun, the underlying logic used to derive it should not differ from the logic used in deriving any of the other propositions of the theory. Yet if we apply the reasoning used in deriving Bueno de Mesquita's proposition 6 to his proposition 9, we see that under some conditions his proposition 9 is inconsistent with his proposition 6. Specifically, if we hold i 's net expected gain constant, the difference between E^i

and E^j is greater for those situations in which j 's expected loss is larger than i 's expected gain. But according to the reasoning employed in the derivation of his proposition 6, when both i and j expect i to win and j sees its losses to be greater than it perceives i 's net gain, j will concede to i in order to prevent war. In his proposition 9, however, Bueno de Mesquita says that in this situation negotiation between i and j will prove most intractable.⁷

A fifth and final problem occurs in the derivation of his (1981) proposition 4. Here, the inference as stated is incomplete. Proposition 4 states that great powers (i.e., major powers) disproportionately initiate long-distance war. But this inference is valid only if the utilities of major power initiators for long-distance wars are greater than or equal to the utilities of non-major-power initiators. Bueno de Mesquita's argument is that in long-distance wars, the probability increases that $E^i < 0$ for weaker nations, while the probability that $E^i \geq 0$ for more powerful nations increases (consequently, weaker nations should be engaged in fewer long-distance wars). The determination of E^i is, in turn, dependent upon national power (or probability of success) and utility. Hence, the derivation that E^i decreases for weaker nations while it increases for more powerful ones in long-distance wars is valid only if the utilities held by major powers are equal to or greater than the utilities held by nonmajor powers. Obviously, if the utilities of nonmajor powers are greater than those of major powers, the expected utility values of nonmajor powers for fighting long-distance wars may be greater than the expected utility values of major powers. If this latter condition is satisfied, his proposition 4 is not a valid inference.

Summing up, Bueno de Mesquita's theory of conflict has generated a number of internal conceptual problems. Several

critics have argued that some of the theory's basic concepts are ambiguous and reductionist. We have argued, as well, that several propositions described as deductions from the theory are not logically valid inferences. We turn now to some of the external conceptual problems generated by the theory.

External Conceptual Problems. The major attacks on Bueno de Mesquita's work have been in the area that Laudan has categorized as external conceptual problems. For Laudan, external conceptual problems arise when the theory under investigation is incompatible with another theory believed to be relevant to the theory under investigation. Both Wagner (1984) and Nicholson (1987) argue that Bueno de Mesquita's formulation of expected utility does not conform to Von Neumann and Morgenstern's (1944) classic formulation of this concept, producing inconsistent consequences according to Wagner, and implausible consequences according to Nicholson. Zagare (1982) criticizes Bueno de Mesquita for making interpersonal comparisons of utility, which is incompatible as well with the classic formulation of utility theory.

External conceptual problems may also result when questions arise about the appropriateness of the methodology used by the scientist. Methodology includes testing procedures, as well as the validity and reliability of operationalizations. Each of these has been the focus of criticisms made against Bueno de Mesquita's work. Majeski and Sylvan (1984), for example, object to his case selection and coding procedures in the testing of hypotheses. Both Wagner and Nicholson argue that the operationalizations employed by Bueno de Mesquita can be used to support rival explanations of conflict as well as, if not better than, his theory. Bueno de Mesquita operationalizes the theoretical concepts of probability and utility as national power capability and

similarity of alliance commitments, respectively. These operationalizations enable him to discuss a wide array of empirical phenomena. But at the same time, they have led Nicholson to conclude that the theory is more plausibly interpreted as confirmation for "hypotheses about capabilities and alliance similarities being good predictors for war initiation" (Nicholson 1987, 366) and Wagner to interpret it as a power politics theory that he claims to have refuted (Wagner 1984). Nicholson (1987) and Majeski and Sylvan (1984) further argue that because many of his tests use inappropriate data, they do not even test the propositions claimed. Khong (1984) and Nicholson (1987) claim that the data refute, rather than support, his major premise that positive expected utility is a necessary condition for war.

Bueno de Mesquita has responded to these different criticisms in different ways. He has dismissed some of them, claiming they are not valid because they result from misinterpretations of his theory. In "The War Trap Revisited," however, he modifies the initial theory in order to eliminate interpersonal comparisons of utility and incorporates risk taking into the utility functions so as to make the theory more parsimonious. He also revises the expected utility formulations to make them conform more closely to the classic theory of expected utility. In general, though, the most ubiquitous defense of his theory is that despite its conceptual problems, it still solves more empirical problems than any known theory in the field.

Empirical Problems: Solved and Anomalous

According to Laudan, Lakatos, and other philosophers of science, to determine whether or not a theory has solved an empirical problem, the analyst must examine the logical structure of the theory as well as the empirical evidence. It is

necessary to examine the theory's logical structure because the theory's empirical problem-solving capabilities depend upon "the extent to which the theory figures in a schema of inference whose conclusion is an approximate statement of the problem" (Laudan 1977, 25). In other words, the theory must logically imply an approximate statement of the problem before the theory can be regarded as having solved the problem. Here it is important to explain what is involved in requiring a theory to imply an "approximate" statement of the problem rather than an "exact" statement of the problem. Although scientific theories may contain either universal generalizations or probabilistic statements as predictions, these theoretical predictions (or laws) apply to an infinite set of observational statements. The relationship expressed by a universal generalization pertains to every observed instantiation of the generalization. A single observation however, often involves some error in measurement. Consequently, if a theoretical prediction is a universal generalization, each observation is required to be consistent with the theoretical prediction within certain well-defined limits; that is, to claim empirical support for theoretical predictions that are universal generalizations, each observed result must be a close approximation of the predicted result. The inferences of probability statements, on the other hand, do not apply to individual instantiations; rather, they apply to a set or class of observations. As a result, to claim empirical support for theoretical predictions that are probabilistic statements, each observed result is not required to approximate the predicted result closely. Significance tests and the like are used to determine exactly how many observations must approximate theoretical predictions in claiming support for the prediction.

For each of Bueno de Mesquita's theoretical derivations, we must therefore

consider logical structure as well as empirical evidence in determining whether his theory solves empirical problems. Specifically, we need to examine the correspondence between the theory's predicted outcomes and observed outcomes to see whether the theory logically implies an approximate statement of the problem. Moreover, because his theory generates both predictions that are universal generalizations and predictions that are probabilistic statements, we need to see whether the appropriate tools of analysis are used in assessing the empirical evidence for each type of prediction. If his theoretical derivation is a probability statement, significance tests are clearly appropriate for determining whether or not this derivation is supported by the data. However, if the derivation is a universal generalization, a single observation (or instantiation) that is not an approximate statement of the theoretical prediction constitutes a refuting instance for the theory. We have demonstrated that several derivations claimed for the theory were not valid inferences. We therefore exclude those inferences from our discussion of the empirical problem-solving capabilities of Bueno de Mesquita's theory.

Using the guidelines mentioned above, Bueno de Mesquita's theory appears to have solved some important empirical problems. Specifically, the empirical problems solved by his theory include his (1985a) proposition 5 and his (1981) proposition 4.⁸ His (1985a) proposition 5 says that when i and j are disputants and each has a negative expected utility for war, the probability of war should approach zero; that is, $E^i < 0$ and $E^j < 0$ then ($p[\text{war}]$ approaches 0). Because this conclusion is a probabilistic statement, the presence of some anomalous cases does not necessarily refute it. In his analysis of data related to this proposition, Bueno de Mesquita finds that in the six disputes where both $E^i < 0$ and $E^j < 0$, war did

not occur, thereby providing empirical support for his proposition 5.⁹

His (1981) proposition 4 states that the initiators of long-distance wars are likely to be more powerful, on average, than the initiators of neighborhood wars. As we indicated earlier, this proposition is valid only if we assume that the utilities of major powers are greater than or equal to the utilities of nonmajor powers. Major powers initiate a disproportionate number of long-distance wars because their expected utilities are less likely to be negative for long-distance wars (than are those of less powerful nations). Their expected utilities are, in turn, less likely to be negative because they have greater probabilities of success (i.e., they are more powerful) than nonmajor powers. We note once again that since this derivation (like his [1985a] proposition 5) is expressed as a probabilistic statement, the presence of some nonmajor powers initiating long-distance wars will not necessarily refute this proposition. Rather, tests of significance are appropriate tools for ascertaining how many anomalous cases are significant. In testing this proposition, Bueno de Mesquita finds that while 22.9% of all wars initiated by major powers were long-distance wars, only 2.4% of wars initiated by nonmajors were long-distance wars. Using tests of significance, he finds this difference significant, indicating support for his proposition 4. His theory thus solves the problem of why more powerful nations are more frequently involved in long-distance wars than are less powerful nations.

While Bueno de Mesquita has clearly solved some empirical problems, he also claims to have solved some problems that remain, in fact, unsolved. Specifically, he claims that his analysis of data related to his (1981) proposition 4 also explains or solves the problem of the greater observed frequency of local wars, as opposed to long-distance wars. But we do not find either logical or empirical support for

claiming that his theory solves this problem. Bueno de Mesquita wants to show that rational considerations alone can explain the greater frequency of local wars better than traditional explanations that emphasize historical or cultural factors. Unlike his proposition 4, this proposition is not a probabilistic statement. Rather, this proposition predicts that rational calculations are made in the initiation of all wars. Therefore, to provide support for this proposition, he needs to show that all long-distance and neighborhood wars are initiated by nations with positive expected utilities. In his analysis of his proposition 4, Bueno de Mesquita finds that all major powers had positive expected utilities in the long-distance wars they initiated. But he does not report either the expected utilities of non-major-power and major power initiators of local wars or the expected utilities of non-major-power initiators of long-distance wars. Unless he can show that all wars are initiated by nations with positive expected utilities, he cannot claim that rational considerations are an alternative explanation for the greater frequency of local wars. In short, the data reported are insufficient for Bueno de Mesquita to conclude that local wars are more frequent because "long distance wars are eliminated as rational possibilities for most actors most of the time" (1981, 84).

The most obvious and critical inference in Bueno de Mesquita's theory of conflict is that leaders with negative expected utilities will not (rationally) initiate war. In testing this proposition he finds that in 76 cases of war 11, or 14%, of war initiators had negative expected utilities.¹⁰ Khong (1984) in particular, regarded these 11 cases as clearly falsifying Bueno de Mesquita's theory. Bueno de Mesquita's reply was that Khong was subscribing to "dogmatic falsificationism," implying that to reject a theory simply because some observations refute the theoretical predictions is irrational and extreme. More

specifically, he argues that "because my operational indicators . . . are imprecise, I recognize that it is unlikely that there will be no exceptions of my theory" (1984b, 126). Unwilling to accept anomalies per se as an indicator of his theory's empirical shortcomings, Bueno de Mesquita suggests instead that his theoretical predictions be subjected to unusually rigorous statistical analysis. He spells out the specifics of the proposed statistical tests as follows: "As a minimal requirement, each deduction from the theory must be supported by evidence falling in the direction predicted by the theory and (when appropriate) must be statistically significant at least at the .025 level. Furthermore, I expect measures of one-way association (that reflect necessary, but not sufficient, conditions) to be as strong as .70 or more. Results weaker than that, though still potentially of interest, cast doubts on the general explanatory power of the theory" (1981, 126). Noting that "these standards are substantially higher than those used in the social sciences" (1984b, 381), Bueno de Mesquita uses Yule's Q to determine whether positive expected utilities characterize initiators while negative expected utilities characterize victims. Additionally, he performs a test of significance to determine whether the expected utility values of initiators and victims differ significantly from what would be expected by chance alone. He concludes from a Yule's Q of .94, that initiators tend to have positive expected utilities, and victims tend to have negative expected utilities.¹¹ From the test of significance, he concludes that these results are not due simply to chance.

Several points need to be made here. First, Bueno de Mesquita has more than adequate support in the philosophy of science literature for his claim that the presence of anomalies does not constitute grounds for rejecting a theory. Every major scientific theory in the history of science has been faced with refuting in-

stances. According to many philosophers of science, these refutations did not constitute grounds for rejecting a theory, nor should they (see, e.g., Kuhn 1962, Lakatos 1970, Laudan 1977, and Musgrave 1974). Indeed, science often advances when scientists maintain faith in a theory in spite of its anomalies and succeed in turning these anomalies into solved problems. Second, as we noted at the outset of our discussion on solved and unsolved problems, significance tests are appropriate *only* for analyzing probabilistic hypotheses. Because Bueno de Mesquita's claim that nations with negative expected utilities will not initiate conflict is a universal generalization, his use of significance tests to analyze the empirical power of this claim is clearly inappropriate.

While other critics have made this same point (Nicholson 1987, 363-66), an example from the physical sciences may serve to demonstrate not only how such tools are inappropriate but also how their improper use obfuscates the opportunity for theoretical advances inherent in anomalous cases. Prout's theory of atomic weights assumed that all elements were composed of hydrogen. The conclusion drawn from this hypothesis was that atomic weights of all elements should be integral multiples of the weight of hydrogen. Scientists found however, that there were several elements that did not conform to this law. Specifically, they found three anomalous elements. If scientists had performed significance tests to determine the relationship between atomic weights of elements and the atomic weight of hydrogen, they would have indeed found a significant relationship in spite of the three anomalous cases. Instead, scientists treated these cases as anomalies and by persisting in their efforts to account for them were able to turn these refutations into empirical successes for Prout's theory (Laudan 1977, 31).

Like Prout, Bueno de Mesquita also

may benefit by treating the 11 cases that do not conform to his theoretical expectations *as if* they were refutations. When, as we noted earlier, a single empirical "observation" involves a whole series of theories, one can never be sure whether a refuting case results from the theory itself (as Khong [1984] argues) or from the associated theories of measurement (as Bueno de Mesquita argues). But as the Prout example suggests, by treating these 11 cases as theoretical refutations, Bueno de Mesquita may ultimately be able to convert them into solved empirical problems for his theory. This would require him, of course, to identify those necessary conditions (or auxiliary assumptions) that account for remaining anomalous instances. As Laudan makes clear, this kind of modification, "which preserves the problem solving capabilities of a theory and saves it from an anomaly . . . requires as much theoretical imagination or luck as the creation of a new theory" (1977, 116-17). If Bueno de Mesquita succeeds here, however, his theory would stipulate necessary conditions for *all wars*, something it presently is unable to do.¹² In short, anomalies may be set aside temporarily with the expectation that they will, at some later point, be turned into corroborations for the theory. But to deny their existence is to miss the opportunity they offer for theoretical progress.

A comprehensive assessment of theoretical progress requires us to identify all possible refutations to Bueno de Mesquita's theoretical predictions. We noted earlier that his (1981) proposition 1, prior to its reformulation as a hypothesis, was a valid inference. Unfortunately, the data Bueno de Mesquita presents regarding this proposition are inadequate to determine whether it is faced with refuting instances. His proposition 1 said that weaker non-aligned nations would not rationally attack more powerful nations. The data indicate that 4 out of 19 (or approximately 20%) of nonaligned nations initiated

war against more powerful nonaligned nations. The sign of the weaker nations' expected utilities is unreported in these four cases. His proposition 1 is not refuted if the scores of these weaker initiator nations are positive. If these signs are negative, however, his proposition 1 has several refuting instances.

There is also inadequate data to determine whether his (1981) proposition 7 is faced with refuting instances. This proposition involves a situation of deterrence where nations i and k are assumed to be major powers. $E^i \geq 0$ against j ; $E^k \geq 0$ against i ; and $E^i < 0$ against k . If these conditions are met, Bueno de Mesquita infers that i will be deterred from attacking j . This inference is valid, however, only if i 's expected utility for attacking both j and k is negative (i.e., if $E^i < 0$ against both j and k). If i 's expected utility against both j and k is positive, we can only infer that i may or may not be deterred from attacking j . In testing his proposition 7, Bueno de Mesquita identifies nine situations in which the conditions he states are satisfied. In three of these nine cases (or 33%), $E^k > 0$ against i , and i was not deterred. If $E^i \geq 0$ against both j and k , these three cases are not refutations. But if i 's expected utility is positive against both j and k , the theory does not imply anything about i 's subsequent behavior. This is because if $E^i \geq 0$, i 's predicted subsequent behavior is tautologous. On the other hand, if $E^i < 0$ against both k and j , these three cases are refuting instances.

Proposition 7 generates actual, as well as potential, anomalies. In concluding the data analysis regarding proposition 7, Bueno de Mesquita notes that "a credible threat, as reflected by k 's positive expected utility score against i , appears to be necessary (but not always sufficient) for successful deterrence" (1981, 172); that is, if deterrence is to be successful against i then $E^k \geq 0$ against i . This statement implies that if $E^k < 0$ against i then

deterrence will be unsuccessful against *i*. Looking at Bueno de Mesquita's data, in 1 of 8 situations (12%), $E^k < 0$ against *i*, and deterrence was successful. This, however, refutes his conclusion. The same percentage of refuting cases occurs when Bueno de Mesquita analyzes James Russett's data, and 6 of 15, or 40%, of the cases using C. Gochman's data refute his conclusion.

Having encountered a number of actual, as well as potential, anomalies in Bueno de Mesquita's theory, it seems advisable to reconsider briefly Bueno de Mesquita's comparison of his theory with other theories of conflict. Such a reconsideration indicates that Bueno de Mesquita has inferred several propositions that were not logical derivations of his theory and then proceeded to compare these invalid inferences to propositions found in the literature on war. Two of the implications he has invalidly drawn from tests of his major premise, for example, are (1) that initiators of war tend to have positive expected utilities and (2) that victims of war tend to have negative expected utilities. But as we pointed out earlier, these are statements of sufficient conditions for war initiation and victimization and cannot logically be inferred from his major premise. Moreover, his theory does not imply anything about the sign of victims' expected utilities. Nonetheless, Bueno de Mesquita compares these invalid inferences to hypotheses drawn from the literature on both the balance of power and alliances, concluding that expected utility, rather than power or alignment, is "the stronger and more stable predictor of the predilection to initiate conflict" (1981, 145; emphasis mine).¹³

In addition to propositions just discussed, Bueno de Mesquita's work also contains many statements that predict the likely winners and losers in international conflict, the costs of war, when countries are likely to terminate an ongoing war,

and when they are likely to negotiate rather than fight. But these propositions are identical to the propositions already discussed in that they assume that positive expected utility is a sufficient condition for war initiation. And, like the propositions already discussed, they are not valid inferences of his theory. Because a theory must logically imply an approximate statement of the problem in order to be regarded as having solved the problem, these additional empirical problems remain unsolved by his theory and are consequently inappropriate to use in comparisons with rival explanations of war. Bueno de Mesquita's use of significance tests and proportionate reduction in error measures to assess the predictive capabilities of positive expected utility vis-à-vis rival explanations, therefore, are tests of rival hypotheses but are not tests of rival theories.

We have identified the theory's solved empirical problems, as well as theoretical predictions that were either unsupported by the data or were faced with refuting instances. While agreeing with Bueno de Mesquita that refutations do not falsify his theory, we have argued that he needs to treat these anomalous cases as if they were theoretical refutations and at some point attempt to modify his theory to eliminate them. We also agreed that an appraisal of his theory requires Bueno de Mesquita to compare his theoretical predictions and the predictions of rival theories. We found, however, that due to inferential errors in his deductions, his comparisons were seriously flawed. Consequently, we are unable to choose between Bueno de Mesquita's theory and rival theories of conflict on the basis of which one has solved more empirical problems. We are nevertheless able to provide an overall evaluation of Bueno de Mesquita's theory in light of our review of the theory's solved empirical problems and unresolved conceptual and anomalous problems.

Appraisal of the Expected Utility Theory of War

Do the conceptual and anomalous problems generated by Bueno de Mesquita's theory so far outweigh its solved problems that we have sufficient grounds for rejecting it? Khong (1984) and Majeski and Sylvan (1984) believe that the problems generated by the theory are more numerous and serious than the empirical problems it solves. They conclude that we should reject the theory along with the expected utility conceptual framework that spawned it. To address these critics, we first need to look more closely at the nature of the relationship between Bueno de Mesquita's theory and the larger, expected utility approach of which his theory is only a part. For Kuhn (1962), the expected utility approach is the paradigm; for Lakatos (1978), it is the research programme; and for Laudan (1977), it is the research tradition. But all three would view the relationship between Bueno de Mesquita's theory of conflict and the expected utility approach as a relationship between a macro or global theory and one of its component parts. Kuhn, Lakatos, and Laudan also agree that in general, decisions to accept or reject focus on global theories rather than their constituent parts. Third, the decision to accept or reject a global theory (or conceptual framework) is made independently of the decision to accept or reject one of its constituent theories. This means it is possible—and in fact oftentimes the case—that an individual theory is rejected but the global theory that spawned it remains acceptable. This discussion strongly suggests that the conclusion reached by Khong and Majeski and Sylvan to reject the expected utility approach is, at the very least, premature.

Just as Kuhn, Lakatos, and Laudan agree that one does not reject a global theory on rejecting one of its constituent theories, so also these authors agree that

the decision to accept or reject an individual (or global) theory is a comparative matter. A major area of disagreement among these philosophers and historians of science, however, involves the appraisal of competing theories. As we noted earlier, for Lakatos decisions to reject and accept research programmes are dependent on the degenerative and progressive qualities of rival programmes. For Kuhn, one paradigm is replaced by another when the former becomes riddled with too many anomalies and the latter has been more successful at resolving some of these anomalies. Unlike either Kuhn or Lakatos, Laudan argues that decisions to accept and reject research traditions are based on the "momentary adequacy" of the tradition. By a more momentarily adequate research tradition he means a research tradition that solves a greater total number of empirical and conceptual problems than its rivals. While accepting the more "momentarily adequate" research tradition, however, Laudan notes that it is also rational to pursue a momentarily less adequate alternative. Specifically, it is rational to pursue a momentarily less adequate alternative when it solves both conceptual and empirical problems at a faster pace than its more adequate rival; that is, it is rational to pursue a less adequate research tradition as measured by a higher rate of progress, even though it doesn't solve as many total problems as does its more adequate rival.

These observations lead us to disagree with critics who maintain that we should reject Bueno de Mesquita's expected utility theory of conflict. The overall assessment of Khong and Majeski and Sylvan, as well as Wagner (1984) and Nicholson (1987), is that Bueno de Mesquita's theory is riddled with sufficient conceptual and empirical problems to justify its rejection. While we agree that Bueno de Mesquita's theory does indeed generate many conceptual and anomalous problems, we

argue against rejecting it. These authors' assessments of Bueno de Mesquita's expected utility theory are not made in comparison with other theories. At the very least, rejection of his theory requires them to demonstrate that some rival theory has fewer anomalies, is more progressive, or is a more efficient problem solver than is the expected utility theory of conflict. Furthermore, rejection implies that the problems generated by Bueno de Mesquita's theory cannot be resolved with subsequent revision and modifications. Yet no one can claim to have such insight into the future.

The current state of theories in international conflict indicates that rational grounds do not yet exist for labeling one theory more momentarily adequate than others. But we believe there are several rational reasons for pursuing Bueno de Mesquita's theory to study conflict. First, this theory relies on deductive reasoning or proofs to generate statements about empirical problems. The use of deductive reasoning and proofs advances theoretical progress both by exposing faulty reasoning and by providing a more guided search for refutations and further conjectures (where conjectures are statements of empirical problems). Lakatos (1976), for example, argues that proofs are in large part responsible for the progress evidenced in the development of mathematical theories. And although many have noted the critical role proofs (or deductive reasoning) play in scientific explanations, Lakatos points out their much-overlooked role in the logic of mathematical discovery. Specifically, Lakatos demonstrates that the use of proofs guided mathematicians in their search for sources of potential refutations as well as in their search for additional empirical problems to solve. Both of these benefits of deductive reasoning are apparent in Bueno de Mesquita's work. By deriving his propositions from a finite set of assumptions, Bueno de Mesquita makes it possible to

demonstrate clearly through logical proofs invalid inferences in his theory. Perhaps even more importantly, the deductive structure of his work guides other researchers to further conjectures.

We noted for example, in our discussion of the theory's conceptual problems, that his (1981) propositions 3, 3', and 3'', when restated as hypotheses (i.e., Alliances increase a nation's chances of being attacked by its friends), were not valid inferences. But if we apply the reasoning used by Bueno de Mesquita to infer these propositions, we can validly infer a slightly different, counterintuitive proposition, that the most antagonistic nations will not fight one another. Bueno de Mesquita infers his proposition 3 as well as 3' and 3'' by assuming that i and j 's alliance commitments are identical. If that is the case, any anticipated change in j 's policy, from i 's perspective, is for the worse. Nation i may therefore initiate a war with j in order to prevent the situation from deteriorating even more in the future; that is, i anticipates $E^i < 0$ changing to $E^i \geq 0$. This is how Bueno de Mesquita infers that allies are more likely to fight than antagonists. Now, in the case of enemies, i 's and j 's policies are assumed to be diametrically opposed. If this is the case, i can only anticipate that any change in j 's policy will be for the better. Using Bueno de Mesquita's reasoning, i may anticipate $E^i \geq 0$ changing to $E^i < 0$ at some future point in time. From this we can infer the obviously counterintuitive conjecture that war will not break out between the most antagonistic nations.

The guided search for conjecture allowed by Bueno de Mesquita's work is a sharp contrast with the more prevalent inductive approach to the study of conflict. In these studies, the empirical problems examined often are products of unguided, ad hoc searches. The argument typically given for this approach is that prior to constructing theories, we must determine which unsolved empirical

problems are facts and therefore in need of explanations or solutions. Researchers adopting this approach believe that they are promoting theoretical progress by identifying the unsolved empirical problems requiring solutions. But the belief that this approach generates theoretical progress rests on the assumption that all known facts equally require solutions or explanations. Typically, however, scientists are most interested in facts that either are solved by theories or facts that refute theoretical predictions.

A second reason for advocating continued pursuit of Bueno de Mesquita's theory is that it evidences a strong positive heuristic. While there are many criticisms of Lakatos' concept of research programmes, several philosophers of science nonetheless concur with Lakatos regarding the role played by the positive heuristic in a progressive research programme. Berkson (1974, 1976) for example, finds that a progressive research programme is marked by a positive heuristic that "guides the researcher to new ideas and problems" (1976, 53). Musgrave, too, finds that progressive research programmes "throw up a rich store of interesting problems and hints about how to solve them, while a degenerating program does not" (1976, 482). Bueno de Mesquita's expected utility approach appears to possess such heuristic power. Bueno de Mesquita, for example, suggests computing expected utility values for diplomacy as an alternative to war in order to determine when leaders should rationally pursue their goals through diplomatic bargaining rather than war. He also proposes the utilization of decision rules other than maximizing expected utility (such as satisficing and minimax strategies and so on) as ways of improving the fit between theoretically predicted results and observed results (1981, 183-84). In one subsequent study Bueno de Mesquita and Lallman (1986) extend the theory to predict situations in which conflicts (i.e., threats

and interventions) escalate into warfare and situations in which conflicts de-escalate into negotiation. In other studies he extends his expected utility model to explain how different types of policy decisions are made and to predict the specific resolution of the issues under consideration (Bueno de Mesquita, Newman, Rabuska 1985). His model, for example, was able to predict shifts in strategy to economic warfare in the Iran-Iraq war (Bueno de Mesquita 1984c). Clearly, Bueno de Mesquita's theory suggests important problems and provides hints about how to solve them. For these reasons, we believe there are rational grounds for pursuing his theory of conflict.

By adopting Laudan's criteria for theoretical progress, we were led to identify invalid inferences in Bueno de Mesquita's theory, as well as to examine all derivations of the theory confronted with refutations. Just as importantly, we were provided a framework for assessing a wide range of diverse criticisms directed at this theory. By helping us to weigh the strengths and weaknesses of Bueno de Mesquita's work, Laudan's framework led us to recommend pursuing Bueno de Mesquita's theory in spite of its obvious flaws. Whether or not one agrees with this recommendation, however, the fact that Laudan's theory of scientific progress was so helpful in evaluating Bueno de Mesquita's work and the wide range of criticism it has provoked indicates that Laudan's criteria has broad applicability to social science theories.

Notes

We are indebted to Charles Andrain and Susan Hekman for their thoughtful suggestions and comments.

1. For more detail on this conference see Rosenau 1973.

2. Bueno de Mesquita in a reply to criticisms by Khong says that the heart of his work "is not that positive expected utility is necessary for conflict

initiation, but rather, that deductive logic allows the specification of counterintuitive circumstances under which conflict is likely to emerge" (1984b, 74)—all the more reason to inspect the application of the rules of logical inference closely.

3. Bueno de Mesquita and Lalman's (1986) theory of conflict escalation specifies necessary and sufficient conditions for war and involves probabilistic derivations about actual phenomena. The assumptions made in this theory differ considerably from those made in *The War Trap* and "The War Trap Revisited." We do not include Bueno de Mesquita and Lalman's theory in part because of its unique assumptions and also because we are unable to assess its empirical-conceptual problem-solving capabilities in its present stage of theoretical development.

4. Wagner (1984) shows that this assumption cannot be derived from the assumption that leaders act to maximize their expected utilities. However, the assumption that positive expected utility is a necessary condition for war is consistent with the assumption that leaders act to maximize their expected utilities, so we include it as an additional assumption of the theory. Moreover, we believe it plays the most critical role in many of the theory's derivations.

5. This form of invalid reasoning is known as the fallacy of denying the antecedent.

6. We realize that our formulation involves interpersonal comparisons of utility. We use the notation subscribed to in Bueno de Mesquita's original model in *The War Trap* for simplicity. In "The War Trap Revisited" Bueno de Mesquita corrects for interpersonal comparisons of utility and changes his notations accordingly. He also indicates that his revised model includes all the derivations described in *The War Trap*.

7. His (1981) proposition 9 also appears to be inconsistent with propositions regarding "resist" and "yield" (Bueno de Mesquita 1985a; Bueno de Mesquita and Lalman 1986). If the relationship between E^i and E^j in his (1981) proposition 9 was expressed as a sum, however, his proposition 9 would be consistent with his (1981) proposition 6.

8. His (1981) proposition 5 is valid; but it is not tested, so we do not include it as a solved empirical problem.

9. We note that the additional knowledge of j 's expected utility is irrelevant for inferring this result; $E^i < 0$ is sufficient for inferring that war will not occur.

10. These figures are from *The War Trap*. In it, conflict data are broken down and reported for three distinct categories: wars, interventions, and threats. Conflict data are not broken down into these three categories in "The War Trap Revisited"; instead, Bueno de Mesquita reports the results of his analysis of 133 conflicts. He finds that 26 initiators had nega-

tive expected utilities. Consequently, the revised theory has refuting instances as well. We are confining ourselves to the test results on war in *The War Trap* for clarity of our argument. Clearly, whatever results pertain to war also pertain to the larger set of conflict data that Bueno de Mesquita examines.

11. Nicholson (1987) notes, however, that Yule's Q is inappropriate for testing Bueno de Mesquita's derivation. The theory implies nothing about the sign of the victim's expected utility, while the computation of Yule's Q involves a comparison of signs of both the initiators' and victims' expected utility scores.

12. Bueno de Mesquita, in his reply to Khong, adds "[in more recent works] I include several auxiliary assumptions which account in a systematic, rather than an ad hoc fashion, for many of the seeming anomalies in my results. For instance, in a recent study I show that many seemingly contradictory conflict initiations occur because of differences in perception between i and j ..." (1984b, 71). However, an examination of Bueno de Mesquita's subsequent studies indicates that while his revisions may address "many seeming anomalies," they do not address—nor do they resolve—the contradiction between the theoretically predicted results and observed results noted by Khong.

13. We additionally believe that Bueno de Mesquita has misrepresented the balance of power theory in his presentation of the hypotheses. Specifically, the balance of power theory incorporates both power and alignments as sufficient conditions for war; but Bueno de Mesquita, in his tests of balance of power and alliances, tests the individual impact of each of these variables on war initiation. Wagner (1984) makes a similar point.

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CHOOSING JUSTICE IN EXPERIMENTAL DEMOCRACIES WITH PRODUCTION

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We examine in a laboratory setting how direct participation in choosing a principle of distributive justice and a tax system impinges on subjects' attitudes and subsequent productivity when they participate in a task, produce income, and then experience losses or gains according to the tax system. Experience with a redistributive principle and its associated taxation system in a production environment does not detract from overall acceptance of the distributive principle, particularly for subjects who participate in choosing the principle. Participation in discussion, choice, and production increases subjects' convictions regarding their preferences. For these subjects (especially recipients of transfers) productivity rises significantly over the course of the experiments. No such effect is evident for subjects who do not participate in setting the regime under which they are to labor. The results' implications for questions of democratic participation and the stability of income support programs are drawn.

One of the most important recent lines of inquiry in political philosophy deals with the question of distributive justice. An influential branch of this literature has used the game-theoretic notion of imperfect information to build on a tradition of "impartial reasoning." These studies have subsumed aspects of distributive justice under questions of fair division. John Rawls's *A Theory of Justice* (1971), the most celebrated of these works, develops an elaborate argument for a particular theory of distributive justice, and his conclusions are typical of this genre. (For an alternative formulation see Harsanyi 1953, 1975.) Rawls argues that under very specific hypothetical conditions (called an "original position") a set of individuals would unanimously choose, as the governing principle of distributive justice, to maximize the welfare¹ of the worst off in-

dividual in the society. Thus, Rawls's theory (and similar analyses) concentrates on the pattern of distribution and deemphasizes questions of entitlements.

By contrast, another set of studies underscores the role of entitlements in questions of distributive justice. A salient example (Nozick 1974) emphasizes fair procedures for maintaining entitlements to the rightful fruit of one's labor. A clear tension exists between these two approaches, in that the latter would regard any requirement to redistribute well-gotten gains as illegitimate, whereas the former would require a degree of redistribution as a minimal requirement of fairness.

In later writings Rawls (1985) shows that he is aware of this tension by setting up the problem of a political conception of justice. He identifies two personal aspects that must be taken into account in

any theory: the capacity for a conception of the good and the capacity for a sense of justice. Within the former is the notion of the *individual's* personal advantage—what one can gain from one's own efforts. The latter deals with moral sentiments regarding what is right from a societal point of view. He notes the potential conflict between the individual's desire for self-enhancement and the need for some form of distributive justice in the society. Redistribution to achieve the latter would be implemented by means of a taxation system.

By identifying that tension, Rawls raises the issue of the potential instability of any principle of distributive justice. Although a principle may appear fair from behind a veil of ignorance, it could begin to chafe in practice when individuals begin to feel its bite. Under taxation, individuals' conceptions of their own good may lead to dissatisfaction. This may lead affected individuals to adduce arguments of entitlements (in line with Nozick's arguments) in seeking to obtain more for themselves. They may begin to question the regime.

This strain poses major problems for modern theories of distributive justice for it raises the specter of the instability of a redistributive scheme. In practice, it has fallen to the democratic process to attempt to resolve these tensions, which manifest themselves operationally in the choice and exercise of the taxation system. Yet if justice in distribution is an important matter, and if it is to be supported in a free and democratic society, a number of political questions have to be addressed. Redistribution almost always involves some form of taxation, so it is necessary to consider how individuals react to an enforcement of redistribution via a taxation mechanism.

Taxation, after all, has played a central role in the evolutions (and revolutions) that have led to the democratization of states in the Western world. Taxation

without representation was a prime motive force for the Magna Carta, the American Declaration of Independence, the French Revolution, and so on. The democratic principle that the population exercise ultimate control over the government's power to tax is considered a serious safeguard against tyranny. The underlying mythology of the democratic creed holds that the process will ultimately lead to a reasonably fair system of taxation.

But in large-scale, modern, representative democracies the single voter exercises that authority indirectly, at a great distance, with minimal information. Indeed, in many democracies, large portions of the franchised population fail to participate in the democratic process in any meaningful way. They take the taxation as it comes and only rarely, when a salient change in taxation rouses them from their "rational ignorance,"² do they react with political consequences for the political competitors in the system. Yet in their workaday lives average workers live intimately with the consequences of the taxation system. A portion of earnings is taxed via various taxes to pay for government services of all sorts. Standard economic reasoning argues that as income tax levels rise, incentives to earn additional income will be reduced and possible gains in productivity will not be realized. Economic growth will suffer. Ultimately, this may even affect the acceptability of governments. This means that any conceptions of justice in distribution are inherently political and raise empirical questions. Yet very little empirical work has examined the questions of the impact of particular rules of distributive justice on productivity and on the continued acceptability of a particular regime.

There have been a number of efforts devoted to examining various aspects of Rawls's arguments as they bear on distributive justice.³ An early attempt to test one particular aspect of Rawls's assumptions

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as it bears on these questions can be found in Brickman 1977. Essays by Greenberg and Cohen (1982) discuss many of the social-psychological theories, issues, and findings bearing on distributive justice, as do Bierhoff, Cohen, and Greenberg (1986) and Deutsch (1985). From different disciplinary perspectives Hochschild (1981) and Soltan (1982) present reviews of some empirical findings and tests that bear on other aspects of distributive justice. Jasso (1980, 1986), building on a tradition of equity theory in sociology, has attempted to represent sentiments regarding justice in mathematical form. More directly addressing points of our concern, Hoffman and Spitzer (1985) have done experiments directly testing subjects' sensitivity to entitlements as a basis for fair distribution. More recently Frohlich, Oppenheimer, and Eavey (1987a, 1987b), Lissowski, Okrasa, and Tyszka (1989) and Bond (1989) have conducted experimental tests of some of the central aspects of Rawls's argument regarding choices of principles of distributive justice.

But none of these analyses addresses the issues of productivity and the continuing viability of a distributive justice system operating in practice. We examine in a laboratory context how direct participation in choosing a principle of distributive justice (and an associated taxation system) from behind a simulated "veil of ignorance" impinges on subjects' attitudes toward their tax system. We also explore the impact on subjects' productivity when they experience the consequences of the tax system in a production environment.

In particular, we address three questions:

1. Is acceptance of the distributive justice principle and taxation system a function of economic experience under the regime imposed by the group?
2. Is productivity affected by the exercise of the taxation rule?
3. Are there differences in the answers to

these questions when the principle and taxation scheme are imposed rather than agreed upon in a participative fashion?

Research Design

In his celebrated *A Theory of Justice* John Rawls (1971) argued strongly that a particular rule of distributive justice is most fair: maximizing the primary goods of the worst-off member of society (the difference principle). In that work, Rawls's justification for his conception of a principle of distributive justice is, at base, contractarian; that is, it is based on empirical assumptions and arguments regarding what representative individuals would do in a hypothetical, idealized, original position. A recent set of experiments (Frohlich, Oppenheimer, and Eavey 1987a, 1987b; Lissowski, Okrasa, and Tyszka 1989) has explicitly attempted to test the validity of some of Rawls's empirical assumptions and the plausibility of his conclusions. In those experiments we tested which principle of distributive justice would be chosen when Rawls's contractarian "original position" was approximated in a laboratory context.⁴ Those experiments showed that it was generally possible to obtain consensus on a single principle of distributive justice as the "most fair" using the methods proposed by Rawls. And there was *one* principle that stood out as the runaway winner (chosen more than 75% of the time). But the principle chosen was not the one anticipated by Rawls. Rather, it was a "mixed principle" that in effect maximized the primary goods of the group as a whole subject to a constraint that the primary goods (or income) of the worst-off individual in the group not be allowed to fall below a predetermined floor. Subjects wanted the society to have a (legitimate) "safety net"; but given that, they wanted producers to enjoy the benefits of their labor.⁵

These experiments were designed to go

beyond an identification of the most desirable principle of distributive justice. Modifying and extending the basic design we used previously (Frohlich, Oppenheimer and Eavey 1987a, 1987b), we constructed the current experiments to test the stability of support for a principle of distributive justice and its associated taxation scheme in an ongoing setting of production. That setting, moreover, allows for the testing of productivity changes in response to experience with production and redistribution.

Subjects were recruited from large undergraduate classes at the University of Maryland, College Park—students who were generally not close friends. Three different treatment groups were established. In two of them individuals were familiarized with principles for redistributing income. They were told that they were to choose a redistributive principle to govern the redistribution of income that they would earn in a latter part of the experiment, but they were not told the nature of the task they would perform. After discussions among themselves, subjects decided collectively on a principle of distributive justice to govern their future taxation scheme. In one variant the choice was by unanimous decision, in the other by majority rule.⁶ They did this in a simulated "original position." Since the nature of the task to be performed was not made known to the subjects prior to their decision, they were effectively unable to estimate their likely future productivity and economic status in the production economy.

A third treatment group was established as a control to determine the impact of discussion and decision on the acceptability of the principle and on subsequent productivity. In this treatment (referred to below as the "imposed" group) the group was given the same introduction to principles of distributive justice but was not allowed to choose the principle and taxation scheme. The principle was im-

posed by the experimenters. The mandated principle was maximizing the average with a floor constraint—the modal choice of the previous experiments.⁷

After the choice or imposition of a principle, all treatment groups were subject to the same regime. Subjects were assigned the task of correcting spelling mistakes in a text excerpted from the works of Talcott Parsons. Each text was arduous (as the reader can easily confirm by reading any random selection from Parsons) and contained spelling errors. Each individual did the same task, and each received wages for his or her individual production. The marginal pay rate had considerable returns to scale, as is apparent in Table 1.

Individuals' outputs were checked; their earnings, taxes, and take-home pay were calculated and reported to them along with the equivalent yearly income flows implied by the earnings. The principle either chosen by the group or imposed upon them was then applied to their earnings and redistribution was carried out in accordance with the principle. Their post-tax payments for that period were calculated and reported to each of them (see Table 1, col. 4). Taxes needed to raise individuals above the floor income were assessed proportionately against the earnings of those who earned more than the floor income. This process of work, pay, and redistribution was carried out three times in each experiment. Measurements of preferences for principles, satisfaction with the principles, and degree of certainty with the ranking of principles were administered at each stage of the experiments.⁸ Repeating the task allowed for an examination of the relationship between experience with taxation on the one hand and (1) attitudes toward the principles and (2) productivity, on the other. An outline of the experimental structure and the points at which each of the relevant variables was measured is indicated in the Appendix.

The recording of subjects' rankings of

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Table 1. Pay Rates (\$)

Total Number of Errors Corrected	Total Earnings ^a	Equivalent Yearly Salary
1	.25	\$ 2,080
2	.50	4,160
3	.75	6,240
4	1.00	8,320
5	2.50	20,800
6	4.00	33,280
7	5.50	45,760
8	7.00	58,240
9	10.00	83,200
10	13.00	108,160
11	16.00	133,120
12	19.00	158,080
13	22.00	183,040
14	25.00	208,000
15	28.00	232,960

^aAt .25 for the first to fourth errors corrected, 1.50 for the fifth to ninth, and 3.00 for the tenth and up. Note that in the first two experiments the rates were \$.50, \$1.00 and \$2.00. We changed the rates thereafter to increase the number of recipients of low earned income.

principles of redistributive justice at each major stage of the experiments allowed for a measure of any possible change in preferences for principles as a result of experimental treatment both across treatments and within treatments. The measurements of subjects' confidence in their rankings permitted a comparable indication of any change in confidence in their preferences for principles. Similarly, elicitation of their degree of satisfaction with the principle at various stages charted their economic experience's impact on their preferences. The number of corrections they found was a direct measure of their status in the economy. Any changes in productivity as a result of experimental treatment or experience over the course of the experiments were directly measurable.

Experimental Findings

Before reporting the findings, it is instructive to get a preliminary sense of why so many groups favored one principle over all others and what sorts of concerns

they expressed. To do this we excerpted from the conversations subjects had during the course of their deliberations. The discussions that took place in the experimental groups were diverse and wide-ranging. Subjects appeared to take the problem they were charged with seriously, and it seemed that they were engaged in thoughtful reflection about their decisions. The transcripts of their discussions are far too long to quote at length, but a sampling of their arguments may give the reader a feel for the considerations that came into play.

Virtually all groups were concerned that individuals not fall below a certain minimum level of support. The reasons (Frohlich and Oppenheimer 1988) were concern both for the fate of the individual and the welfare of society:

I would like to see that everyone at least has the basic things. After that I don't really care. [If the floor is too low] . . . a lot of people are going to be starving and they will be without shelter and housing. (p. 99)

If you have people that are really poor . . . they have a tendency to just stay there because you

know there isn't enough nutrition, they can't get an education and all these kinds of things. But if you put it on a certain minimum then they have a chance to get out of that situation. They have a chance. (p. 72)

Without a floor . . . you would probably have a lot of crime which would affect everyone's income in terms of insurance, health costs, etc. (p. 2)

They also raised questions of entitlement and of the illegitimacy of redistribution:

SUBJECT 1: So the people that make more money have to support the people that don't do anything.

SUBJECT 2: That's the way it is.

SUBJECT 1: That's not fair . . . I should get what I worked [for] and what I deserve. I don't think I should have to pay for some bum. (p. 147)

In the discussions subjects often cited the potentially corrosive effect on the incentives of redistribution: "But one other thing is here to maximize the productivity too, and you need to have some kind of incentive there to work hard" (p. 116). But most groups in the choice experiments chose a compromise principle that took into account both entitlements and concern for those at the bottom of the income scale: "Maximize the average with a floor constraint? That's the one that I kind of like too. I like it because you're guaranteed a certain amount of money so you're not going to walk away empty. But then if you have to do harder work or more work, you have a chance to maximize your profits" (p. 138). One concern regarding the experimental design was that subjects truly be behind a "reasonably" thick veil regarding their likely standing in the production economy they were to occupy. The verisimilitude of their deliberations, and the thickness of the veil regarding their ultimate position in society is reinforced by some of their comments:

The reason why I'm suggesting what I'm suggesting is that in this particular instance I would assume that our goal as a group is for each of us to get out of here with as much money as possible. Is that a rational assumption in order for all of us to get out of here with as much money as possible and since we don't know what the skills are? I mean out in society I am fairly confident that I could get a fairly good amount of money, but the skills out there are the skills that I am aware of that I have and can deliver. We don't know what the skills are here. So I might be able to make the maximum amount, then again my skills may be such that I won't be able to make a penny here under a normal system. So if I can't make any money with my skills, I would like to be able to make some money by insuring that there is some system that we all get out of here with something more than we came in with. (p. 129)⁹

Although these brief excerpts are a very small selection of the total conversations, from them it should be apparent that the subjects took the exercise seriously and that substantive concerns were voiced.¹⁰

The major treatment variable in the analysis under consideration here was the degree of participation by the subjects in their choice of a principle and taxation scheme. The results of the group choices are represented in Table 2. In the treatment groups in which discussion and choice took place, the most popular group choice was the principle of maximizing the average with a floor constraint. Of the 18 groups 11 chose that principle outright while 4 groups combined it with a range constraint. Only three groups chose the principle of maximizing the average with a range constraint. These results are roughly comparable to those obtained in previous experiments (Frohlich, Oppenheimer, and Eavey 1987a, 1987b) except for the absence of group choices of the principle of maximizing the average income. The principle imposed by the experimenters was maximizing the average with a floor constraint of 99 hundred dollars. The floor imposed was the mean of earlier choice experiments cited above and conformed closely to the mean of the

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Table 2. Group Choice of Principle

Type of Experiment	Principle Chosen		Imposed	Total
	Floor Constraint	Range Constraint		
Choice	15	3	0	18
Imposed	0	0	10	10
Total	15	3	10	28

floors chosen by the subjects in the experiments reported here.

Stability of Preference over Production and Redistribution Periods

Is an individual's acceptance of a principle of distributive justice adversely affected by his or her economic experience of production and redistribution? Our experiments provide a number of measures of this possible effect. Here we report on changes in the subjects' rankings of distributive justice principles over time, subjects' confidence in their rankings, and their satisfaction with the principle under which they were laboring. In the following three tables we examine the results of these measures for all groups in the experiments.

Table 3 reports the overall changes in scores of the subjects' first-place rankings of principles over the period in which they were involved in production.¹¹ Notice that among the 129 subjects there are only

16 gross changes in first-place rankings (the off-diagonal elements), which results in only five net changes. *Maximizing the average* gains five first-place rankings, *floor constraint* loses three, and *maximizing the floor* loses two. While no baseline for the statistical significance of these changes is available, the small number of net changes (5 of 129, roughly 4%) is prima facie evidence of considerable stability. Certainly an erosion of 3 of 81 first-place rankings (3.7%) of floor constraint cannot be construed as a serious erosion of support or an indication of alienation.

Nor is this stability confined to one treatment group in the experiments. When the subjects who chose their principle are examined separately and are compared with those who had it imposed, both groups behaved similarly. For subjects in the choice experiments the number of changes in first-place rankings is 7 of 79 possible. With choice the floor constraint lost 2 of 56 first-place rankings (3.6%). In the imposed experiments there were 9 of a possible 50 changes in first-place rankings

Table 3. Changes in First-Place Rankings of Principles

Start of Production	End of Production Periods				Total
	Maximize Floor	Maximize Average	Floor Constraint	Range Constraint	
Maximize floor	3	0	2	1	6
Maximize average	0	21	2	0	23
Floor constraint	0	6	73	2	81
Range constraint	1	1	1	16	19
Total	4	28	78	19	129

**Table 4. Impact of Production and Redistribution on
Subjects' Confidence in Rankings by Experimental Treatment**

Experiments	Mean Certainty before Tasks	Mean Certainty after Tasks	n	t-score	p
All	3.78	3.96	119	2.55	.012
Imposed	3.46	3.67	49	1.02	.315
Unanimity	3.95	4.19	46	3.00	.004
Majority rule	4.04	4.04	24	.00	1.000

and floor constraint lost 1 of 25 first-place rankings (4%).¹²

Security of Rankings of Principles As a Function of Economic Experience

In addition to actually recording the rankings of each of the principles at various stages by each of the subjects, we also asked how sure they were about their rankings. We can use changes in this measure to see whether their experience increased or decreased their convictions.¹³ The scores for their answers directly after the group choice (or imposition) were compared with the scores at the end of all tasks and redistribution to identify the production period's impact on their convictions. Table 4 gives the results of that analysis.

It is clear from the table that for the group as a whole the production experience had a positive impact on the certainty of their convictions. They are more secure after production and redistribution than they were before that experience. But a breakdown of the data into treatment groups shows that the effect is not at all uniform.¹⁴ In the unanimity experiments, where all subjects had to agree to the principle there is a strong positive impact on subjects' security regarding their ranking of principles. For subjects in the majority rule experiments however, this is not the case. They exhibit no change in their conviction. The subjects in the imposed rule experiments experienced an increase in the security of their rankings, but it was not

statistically significant. Discussion and agreement via consensus does matter with regard to reinforcement of subjects' subsequent security of preference, but discussion and agreement without consensus does not. Thus, discussion and agreement alone do not appear to account for the effect. The requirement of a consensus decision rule has an impact. This difference between the two types of choice experiments should be borne in mind in light of the widespread finding in psychological and social science experiments of the salutary effect of group discussion on subjects.¹⁵

Satisfaction with the Principle As a Function of Economic Experience

What of our third indicator of stability, satisfaction? We specifically asked the subjects, before each production period and then after the last one, how satisfied they were with the principle under which they were operating.¹⁶ Data from those responses can be used to test concerns (such as Nozick's) that a principle of distributive justice like Rawls's would suffer eroding support, making it ultimately unstable. In terms of our experiment, this is what might be termed an "alienation hypothesis": "The satisfaction with the group's decision, made behind the veil of ignorance, will go down after leaving it as subjects begin to experience redistribution."

Table 5 reports the overall changes in scores of the subjects' self-reported satis-

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Table 5. Satisfaction after the Veil Was Lifted

Number of Cases and Mean	Before First Task	After First Task	After Second Task	After Third Task
N	94	93	97	125
Mean	3.80	3.84	3.85	3.85

faction with the principles over the periods in which they were involved in production.¹⁷

It is quite apparent that the data fail to support the alienation hypothesis. Indeed, the evidence bolsters the converse—increasing satisfaction and acceptability. Although the changes are not significant, average satisfaction increases over the production periods. Indeed, in a separate analysis of subjects in the choice versus the nonchoice experiments, the increasing support is much clearer for the choice experiments, but the alienation hypothesis doesn't find statistical support even in the cases where the group's principle was imposed. Nevertheless, some differences between these treatment groups deserve comment. Subjects in choice experiments expressed a higher level of satisfaction at the end of the experiments than did subjects in the imposed experiments. Their mean satisfaction was 3.95 ($N = 87$) while the others had a mean of 3.60 ($N = 42$) for a t -value of 1.95 significant at the .05 level.

Impact of Economic Experience on Productivity

Let us now address one other main question: Would productivity be affected

by the exercise of the distribution-taxation rule, and are any such effects a function of participation in the taxation decision?

The basic measure of productivity was the actual number of mistakes found and corrected by subjects at each of the three production phases. Changes in productivity using these measures were then evaluated. Consider Table 6. It is clear from the first line of the table that for the full set of experiments, experience with production and redistribution *increased* productivity. Average production rose significantly between the first and last periods. This is in flat contradiction to the drop anticipated by an alienation effect. There is, however, at least one possible explanation that would save the hypothesis that redistribution, sapping incentives, should lead to reduced productivity. Since the tasks performed were repeated with different texts, subjects' experience with the task might be expected to increase their productivity as they progressed along a learning curve. After all, subjects might be expected to increase their efficiency as they became more familiar with the demands and mechanics of what they were doing.

However, the breakdown of the subjects into choice and imposed treatment

Table 6. Changes in Production by Experimental Treatment

Experiments	Mean Production		F	p	n
	First Period	Last Period			
All	5.78	6.65	9.87	.002	135
Choice	5.82	7.02	10.92	.001	85
Imposed rule	5.70	6.02	.58	.451	50

Table 7. Impact of Experimental Treatment on Production:
Choice vs. Imposition

Production	Choice Experiment Mean	Imposed Experiment Mean	F	p	n
First	5.82	5.70	.035	.852	140
Last	7.02	6.02	3.241	.074	135

groups negates this explanation as a basis for the observed increase in productivity. The second line of the table shows that the increase in productivity found in the group as a whole is found in greater measure in the subjects who engaged in discussion and choice of a taxation scheme. The third line shows that the subjects who participated in the imposed rule experiments experienced only a marginal and statistically insignificant increase in productivity. Whatever effect experience may have on increasing productivity, it is vitiated by working under an imposed regime. Conversely, it is manifest and potentially augmented when the regime is a matter of participatory choice. The discussions and choice behind the simulated "veil of ignorance" appear to make a difference in subsequent performance.

A direct test of this experimental treatment can be obtained from an analysis of variance using the experimental treatment as a categorical variable. Are the differences in productivity significant between the subjects who discussed and chose a principle and those who simply had one imposed on them by the experimenter? Comparing the first and last period productivity for the two experimental treatments in Table 7 shows that the difference in average production between the two groups is eight times larger in the last period (7.02 versus 6.02) than in the first period (5.82 versus 5.7). Are these differences significant? Table 7 allows us to answer that question.

Clearly the experimental treatment has no effect on the *initial* performance of the subjects. At the beginning of the first pro-

duction period, subjects in the choice experiments and those in the imposed rule experiments performed almost identically. Discussion and choice as opposed to the experimenters' imposition of a rule, seems to make no *initial* difference to their performance.

Subjects' performance in the *last* period tell a rather different story. Here the F-statistic for the experimental treatment begins to approach significance ($F = 3.24$, $p = .07$), although the effect appears not to be particularly strong. Thus, the experimental treatment appears to have had an impact on productivity *after the subjects experienced the consequences of working under the rule*, but the effect appears not to have been particularly strong.

Taxpayers and Recipients: The Impact of Different Experiences on Productivity

Given the indications that discussion and choice versus imposition has an impact on productivity, one is led to ask whether other variables help us understand differences in productivity. Are some subgroups of subjects more affected by the experimental treatment than others, since participants in the experiments experience the effects of redistribution differently? Some are taxpayers, some recipients of transfers. Does one's status as either taxpayer or transfer recipient have a differential impact on the subsequent production behavior? And is this relationship also affected by whether the individual had participated in the process

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Table 8. Changes in Production: All Experiments by Tax Status

Tax Status	First Mean	Last Mean	F	p	n
Taxpayers					
Production	7.51	8.23	4.08	.047	71
Production ratio	.27	.25	1.45	.232	71
Recipients					
Production	3.49	4.58	5.35	.024	59
Production ratio	.12	.14	1.60	.211	59

of choosing the rule that governed redistribution of earnings?

Subjects were grouped by tax status as net taxpayers or net recipient's of transfers (over the whole experiment).¹⁸ To test for changes in productivity relative to the group, each individual's ratio of the group's production in the first and last periods was calculated. This ratio serves as a measure of the relative burden borne by the two economic groups: taxpayer versus recipient. Changes in these ratios should reflect changes in the relative productivity of taxpayers and recipients over the course of the experiments.

The data in Table-8 indicate that in the experiment as a whole, whether a subject is a taxpayer or not, productivity appears

to rise, although recipients' production appears to rise more sharply. Moreover, there is no apparent significant difference in the share of burdens borne by taxpayers and recipients over the course of the experiments. The ratios of their group's production appears relatively stable.

But a different picture emerges when the subjects are further subdivided into experimental treatment groups. Table 9 shows the analysis of changes in productivity for taxpayers and transfer recipients in the choice experiments as opposed to in the imposed experiments. The trends toward increasing productivity identified for all experiments is reflected in the choice experiments in both subsets of par-

Table 9. Changes in Production by Tax Status and Experimental Treatment

Tax Status and Experimental Treatment	First Mean	Last Mean	F	p	n
Choice Experiments					
Taxpayers					
Production	7.57	8.32	3.11	.084	47
Production ratio	.27	.23	4.86	.033	47
Recipients					
Production	3.66	5.42	8.17	.007	38
Production ratio	.12	.16	4.48	.041	38
Imposed Experiments					
Taxpayers					
Production	7.52	8.17	1.35	.255	29
Production ratio	.26	.27	.35	.558	29
Recipients					
Production	3.20	3.05	.05	.824	21
Production ratio	.13	.11	.79	.385	21

ticipants, although some differences emerge.

Note that in the choice experiments the increase in productivity is lower among taxpayers than among recipients. Hence, transfer recipients become relatively more productive members of their experimental group as time goes on. For taxpayers the increase in production of corrections is significant only at the .08 level. For recipients, however, the gain in production is highly significant both substantively and statistically. Gains in production of corrections are almost 50%. This gain is statistically significant at the .01 level. This differential increase in productivity between the two groups leads to an increased relative sharing of the burden of production by welfare recipients over the course of the production runs. Their *per capita share* of production rises significantly from 11.8% during the first period to 15.9% during the last.

These gains in productivity in the choice experiments can be sharply contrasted with an absence of the same effect in the imposed experiments. The picture in the imposed experiments is quite different. There taxpayers increase their production of corrections in a manner reminiscent of the previous result (but now statistically insignificant), while recipients show a very small and insignificant *drop* in productivity.¹⁹ These small changes within groups lead to no significant difference in the ratio of the burden born by the two groups, although the recipients appear tending toward *diminishing* their relative performance.

It is important to note the effect of time on the behavior of the two groups. As time passes, subjects in the two treatment groups react differently to their experiences with the redistributive system. From Table 9 we know that recipients in the choice treatment increased their output significantly over the course of the experiment, while those in the imposed treatment did not. A direct measure of last-phase production differences between

the treatment groups is given by the F-statistics for differences between the two treatments in the last round of production. They are highly significant ($F = 10.43$, $p = .002$). Similarly, the differences in the ratio of recipients' production to the total group production varied significantly across experimental treatments. Recipients in the choice treatment increased their productivity in absolute terms not only, but also relative to taxpayers in their treatment group.

The implicit differences in the behavior of participants in the choice versus imposed rule experiments can be tested directly by an analysis of variance using the experimental treatment as a dichotomous variable. The first line of data in Table 10 deals with the productivity of recipients in the last production period. It shows the significance of the experimental treatment: choice and discussion versus imposition of a principle. The impact of the treatment variable is clear and unequivocal. Recipients who discussed the principles and participated in a choice outperform their counterparts in the imposed experiments by a wide and significant margin. Moreover, they account for a larger share of their group's production. Their production ratio is 50% higher than recipients in the imposed experiments. The second half of the table deals with net taxpayers. There the effect of the treatment is not at all evident. Discussion and choice versus imposition seems not to make a difference in the performance of taxpayers. The only significant difference is that in the choice experiments the average taxpayer bears a lower proportion of the burden of production (23.7%) than his or her counterpart in the imposed experiments (26.9%). But that difference is attributable to the increase in productivity of recipients in the choice experiments.

It is important to recall that there was no significant difference in the *initial* measures of productivity for either recipients or taxpayers across treatment groups.²⁰ Thus, the improved perform-

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Table 10. Impact of Experimental Treatment on Final Production by Tax Status

Tax Status	Choice	Imposed	F	p	n
Recipients					
Last production	5.42	3.05	10.434	.002	59
Last production ratio	.16	.11	7.039	.010	59
Taxpayers					
Last production	8.32	8.17	.219	.641	71
Last production ratio	.23	.27	7.676	.007	71

ance among recipients in the choice experiments cannot be attributed solely to a discussion effect. It emerges only after subjects have experienced production and redistribution.

Thus, the choice-imposition variable does not act uniformly on the classes created by the tax system. This is further evidence that the effect is not solely engendered by discussion. The content of the decision and the status of the subject in the environment after the veil is lifted makes a difference. Initial lack of difference and subsequent large differences only for transfer recipients give further force to the argument that the observed differences are not solely due to the treatment variable (participation in the discussion-and-decision phase) but are a product of an interaction of the treatment variable and the individual's status under the implementation of the redistributive principle.

Discussion

The results of these experiments offer tentative answers to the questions posed at the outset. Acceptance of the taxation principle is not adversely affected by the economic experience of subjects after the veil is lifted. There is no evidence for an undermining of the principle as subjects experience its effects. Guaranteeing a subsistence income to those who cannot earn that level of reward does not undermine the legitimacy of the system in the short

run as measured by rankings of principles, security regarding one's rankings, or satisfaction. However, when subjects participate in choosing the principle, their satisfaction is higher than when it is imposed upon them. When they participate and are required to choose *unanimously*, their confidence in the choice increases. This is not the case when they choose via majority rule.

The floor constraint principle appears to be a robust choice when experienced in practice in a production environment. Its first-place ranking is unchallenged at the end. In summary, acceptance of the principle is, if anything, enhanced by the production and redistribution experience. But these effects appear to be stronger in the choice experiments. Participation in discussion and choice makes a difference in subsequent attitudes and changes in attitudes.

This short-run acceptability of the principle across all groups must be tempered by an underlying trend in productivity. Productivity shows a tendency to increase in taxpayers in all treatments and shows a substantial increase among recipients in the choice experiments. By contrast, recipients in the imposed experiments seem to decrease their productivity. At least for recipients of transfers, participation in a group discussion behind a veil of ignorance, participation in choosing a redistributive principle, *and experience with the economic consequences of the principle* seem to make a difference. Transfer recipients' productivity goes up signifi-

cantly over time. Rather than acting as a disincentive, the taxation and redistribution system that guarantees a floor *when agreed to in a participative fashion* seems to spur those at the bottom of the income distribution to increase their effort and output. Participatory democracy (at least in an experimental setting) aids productivity.

The discussions behind the simulated veil parallel and throw into clear relief the widespread political debates in Western democracies regarding the appropriate limits for the welfare state as opposed to a laissez faire economy. The results reaffirm that subjects can reach consensus on a principle behind a simulated veil of ignorance. Concern for the poor and weak, a desire to recognize entitlements, and sensitivity to the need for incentives to maintain productivity all enter into subjects' deliberations regarding a fair rule for implementing distributive justice. The choice of, and continued support for, the floor constraint principle demonstrates how it serves as a compromise incorporating these competing normative and empirical demands.

Moreover, the effect of the experimental treatment on productivity offers an insight into how a redistributive regime can be rendered stable. There are two themes that pervade the arguments of both politicians and citizens who attack income support programs: (1) An income support program will sap incentives among recipients and will make them dependent and unproductive; (2) It will create a moral hazard and attract the "undeserving"—the "welfare cheaters"—which is unfair to all those who work and ultimately pay for the programs. At least within small groups, explicit discussion of what constitutes fairness establishes the need for, and legitimacy of, *some* sort of support program. When those who ultimately are to be recipients of transfers participate actively in the decision, the dreaded result of increasing dependence and sloth do not

materialize. Quite the contrary. It appears that recipients who actively participate in the decision recognize the entitlement claims of those who are paying for transfer payments and almost literally redouble their efforts to pull their own weight. By contrast, when recipients function under a (functionally identical) income support system that has been imposed upon them and in which they have had no substantial say, they appear to free ride. Their efforts appear to flag and their productivity shows a declining trend. They appear to view the transfers they receive as their due, and they do not make efforts to become self-sustaining. Genuine participation and debate in determining the content of programs appears necessary to prevent the undermining of income support programs.

Of course, there are enormous barriers to the implementation, writ large, of the kind of process that we have set up in the laboratory. But the data point to a need for more informed debate and discussion. For without that participation, the fears of income support critics might be well founded. This need for broader participation is all the more important, given the well-known propensity in democratic systems to decreased political activity with decreased socioeconomic status.

Apart from the experimental results, the possible efficacy of involving workers in decisions is demonstrated by the evidence emerging from the industrial relations literature. Profit-sharing schemes, participation in workplace quality of life issues, the use of quality circles, and a general openness of management to the suggestions from the shop floor are all beginning to show promise of increasing worker productivity. The necessity of participation in such schemes is reflected in Kanter 1987 (p. 32): "For gainsharing plans to work, a particular organizational structure and corporate culture are required—and these include an open discussion of the plan to gain employee accept-

ance, the establishment of cross-unit teams or task forces to develop the plan, and the adoption of suggestion systems."

A moment's reflection on the underlying dynamics of the production environment in the experiments and, indeed, on the production teams operating in industry reveals that they represent "social dilemmas." There are incentives for the least productive to free ride and accept their guaranteed payoffs or their share of the productivity gains of others' efforts. The extensive literature on cooperation in social dilemmas reveals that discussion (especially relevant discussion) increases cooperation in social dilemmas (Dawes 1980; Dawes, McTavish, and Shaklee 1977; Messick and Brewer 1983). From that perspective it should not be surprising that increased participation in determining the content of the reward structure should increase the efforts of those who otherwise would have an incentive to free ride. Indeed, any income redistribution system can be seen to incorporate elements of social dilemmas; so potential solutions that are effective in overcoming free rider tendencies there are of interest in insulating redistributive systems from deleterious effects.

One should not go too far in interpreting these results. The laboratory setting requires a compression of time, income, and stakes and involves small groups. And their subjects, after all, are university students. Nevertheless, the results provide an argument for meaningful participation in the democratic process which goes beyond the simple requirements of informed decision making and fairness. Meaningful participation on a broader scale might well have beneficial implications for the productivity of the economy. It is not all clear how such participation might be accomplished, but its potential impact appears to be worth examining.

Appendix: Sequence of Events In the Experiments

1. oral recruitment of five subjects in large university classes
2. orientation to the problem of choosing a principle of distributive justice
3. measurement of ranking of principles and degree of conviction regarding those rankings
4. reading about implications of the principles and testing (and retesting when needed) regarding the nature and implications of the principles
5. measurement of ranking, etc.
6. group discussion and choice (unanimous, or majority rule), or imposition
7. measurement of ranking, etc. and of satisfaction regarding principle chosen
8. first production period—gross production (TSK1YP), associated income, and net redistributed income reported
9. measurement of rankings, etc.
10. second production period—production (TSK2YP) and incomes reported
11. measurement of rankings, etc.
12. third production period—production (TSK3YP) and incomes reported
13. measurement of rankings, etc.
14. questionnaire

The variables TSK1YP-TSK3YP refer to the subject's production of corrections in TaSK1-TaSK3.

Notes

We thank the National Science Foundation (Grant No. SES 82 07807, A01), the Social Sciences and Humanities Research Council of Canada, and our respective universities for providing support for this research. Howard Harmatz, Jennifer Hochschild, Alphons van de Kragt, Jane Mansbridge, Douglas Rae, and Zowie Wharton provided helpful comments on earlier drafts. We also thank the research assistants who helped run experiments, comment on prototypes, scout out relevant materials and help check our analyses. Irvin Boschman, Michael Cain, Judy Chipperfield, Craig Connors, Pam Edwards, Valerie Lehr, Karen Loewen, and Paul Parker were immeasurably helpful.

1. Rawls introduces the notion of "primary goods" and discusses his principle in terms of maximizing

the primary goods available to the worst-off individual. We use the terms *welfare* and *income* to represent these goods.

2. The term *rational ignorance* was first coined by Anthony Downs (1957) in his ground-breaking book. It refers to the incentives individual voters have to limit their efforts in gathering political information, given the small differences they can (individually) make in the political process.

3. The analytic examination of Rawls's arguments is voluminous. Yearly citations of Rawls run to the hundreds. We, however, are primarily concerned with the application of empirical methods to questions raised by his arguments.

4. A fuller description of the rationale for this methodology is presented in Frohlich and Oppenheimer (1989).

5. That finding is consistent with an earlier theoretical result of Howe and Roemer (1981). They modeled the problem of a choice of a distributive principle behind the "veil of ignorance" as a game and concluded that the choice of distributive principle would be a function of the degree of risk aversion among the individuals. One implication of their analysis is that if the representative individuals who are charged with choosing a principle have a variety of degrees of risk aversion, the principle or principles in the core would be mixed. Thus, there is both empirical and theoretical reason to expect the choice of a mixed principle.

6. In most of the results reported below the two choice groups unanimity and majority rule are treated as a single group, since the particular decision rule was found to have insignificant impact on some variables. Where significant differences occur, they are reported separately.

7. The actual floor imposed was the average of the floors chosen in the experiments cited above.

8. Satisfaction with the "group's choice of principle" was measured only after the last production period in the imposed experiments.

9. A debriefing questionnaire was administered at the end of the last production period to gather data on characteristics of the subjects. No factors were found to have strong explanatory power regarding preferences for, or choices of, principles of distributive justice. Some, however, were able to explain variance in the level of the floor constraint chosen. All in all, however, the "thickness" of the induced veil is always a plausible source of experimental error.

10. Indeed, a reading of the full transcripts gives one an overwhelming sense of the seriousness and substantive nature of the deliberations. A full copy of the conversations, (as well as all other experimental data) either on paper or as a flat ASCII file is available for further analysis for the cost of electronic duplication plus postage.

11. Recall that the ranking of the principles just prior to the start of production is made immediately

after the group choice (or imposition) of the principle, while the final ranking takes place at the conclusion of the experiment after three rounds of production and redistribution.

12. It should be noted, however, that the net number of changes in first-place rankings for all principles seemed to vary by whether the group was permitted to choose. In the choice groups there were four net changes while in the imposed rule groups there were eight net changes, indicating somewhat greater stability in the choice groups.

13. The actual question was, "How do you feel about your ranking of these principles?" And the response code was *Very unsure, unsure, no opinion, sure, very sure*. Weights of one to five were assigned to the categories with a higher value corresponding to greater security.

14. Here the responses of subjects in the unanimity experiments and in the majority rule experiments are presented separately rather than as one "choice experiment" group. The reason for doing so is that the two groups showed differences in the dependent variable in question here.

15. Below we will identify other results that point to the conclusion that discussion per se cannot account for many of the differences between treatment groups.

16. The exact question read, "How satisfied are you with the distributive principle selected by (for) the group?" The responses were *very unsatisfied, unsatisfied, neither satisfied nor unsatisfied, satisfied, and very satisfied*. Satisfaction was scored one to five, respectively. Unfortunately responses to this question were not elicited in 7 of the 10 groups in the imposed treatment.

17. Note that the first measure of satisfaction with the principle is taken immediately after the group choice (or imposition), while each subsequent measure is taken after one round of task completion.

18. It should be noted that other ways of implementing the concept of taxpayer status do not lead to more than marginal changes in the conclusions supported by the data analysis.

19. One possible explanation for the greater increase in productivity of recipients in the choice experiments is that the taxpayers were already producing at such a high rate that there was little room for improvement. That explanation is negated by the finding that in the imposed experiments the productivity of the taxpayers increased while that of the recipients decreased.

20. The F-statistics for experimental treatment effect on recipients' productivity is minuscule for the first round of production ($F = .39$, significant at the .53 level). For taxpayers it is vanishingly small.

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RESEARCH NOTES

THE POLITICAL CONSEQUENCES OF ELECTORAL LAWS, 1945-85

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A systematic analysis of the relationships between the main electoral system variables (electoral formula, district magnitude, and ballot structure) and electoral outcomes (the degrees of disproportionality and multipartism) in the 20 Western democracies from 1945 to 1985—representing 32 distinct electoral systems (an electoral system being defined as a set of elections held under basically the same rules)—shows that the effects of both formula and magnitude on proportionality are very strong, much stronger than Douglas W. Rae and subsequent researchers have suggested; that on the other hand, their effects on the number of parties participating in elections is surprisingly weak; and that ballot structure affects the degree of multipartism only in single-member district systems. These findings suggest that strategic behavior by politicians and voters plays a less important role in reducing multipartism than is usually assumed.

When the first edition of Douglas W. Rae's *The Political Consequences of Electoral Laws* was published in 1967, it was almost instantly recognized as a great scholarly breakthrough. A second edition was published in 1971, and it has maintained its stellar reputation as the most important book in the field of comparative electoral systems ever since. More recent major innovative books have explicitly used it both as a model and as a point of departure (Katz 1980, 20; Nohlen 1978, 14), and contemporary authors writing on the subject still unfailingly cite it. It received the 1989 George H. Hallett Award of the American Political Science Association's Representation and Electoral Systems section.

As the first systematic broadly comparative study of electoral systems and as a powerful stimulus to subsequent research, it clearly deserves its reputation as a classic in the field. On the other hand, it has been accorded this status without suf-

ficient critical attention. In the preface to the second edition, Rae (1971, vii) himself chides "the over-gentle colleagues who reviewed the original edition."¹ In fact, the book suffers from a variety of conceptual, methodological, and empirical weaknesses, which are so grave and numerous that they cast doubt on all of Rae's conclusions. Moreover, some of his conclusions are so surprising—especially the finding that differences in proportional representation (PR) formulas have only minor effects and that neither two-tier districting systems nor the ballot structure have any effect at all—that they cry out for a critical new examination.

My purpose here is to reanalyze Rae's classic study with more accurate data, stronger hypotheses, and better methods. I shall also use *more* data: to Rae's elections (of the lower or only houses of the legislature) in the 20 Western democracies during the 20-year period from 1945 to 1964, I shall add the few elections in

AMERICAN POLITICAL SCIENCE REVIEW
VOLUME 84 NO. 2 JUNE 1990

1945–64 missing from Rae's book and, more importantly, all elections in these countries during the subsequent 21-year period from 1965 through 1985.² In order to save space and because I found no major differences between the patterns in Rae's 1945–64 period and in the entire 1945–85 period, I shall present the results for the latter only.

My reanalysis and update will yield conclusions that are significantly different from the relationships that Rae finds between his variables. Rae defines three aspects of the electoral system as his independent variables: (1) the electoral formula, such as plurality, various forms of PR, and so on; (2) the average district magnitude, that is, the average number of legislators elected per district; and (3) the ballot structure, which may or may not allow the voters to split their votes between two or more parties (pp. 15–46). His two dependent variables are (1) the proportionality of the electoral outcome, which is the immediate or (in Rae's terminology) "proximal" effect of the electoral system's translation of votes into seats; and (2) the degree of multipartism that is—or is presumed to be—the indirect or "distal" effect of the electoral system (pp. 47–64, 67–68, 84–85).

Rae examines five major relationships between his independent and dependent variables. With regard to all five of these, my conclusions will diverge from Rae's in crucial respects: (1) the relationship between the electoral formula and the proportionality of the election result is much stronger than Rae finds; (2) the link between district magnitude and proportionality is also much stronger, especially as far as concerns the effects of two-tier districting systems (which Rae claims to have no effect); (3) the relationship between the electoral formula and the degree of multipartism is much weaker than in Rae's finding; (4) similarly, district magnitude and multipartism are much less strongly and perfectly related than Rae claims; (5)

the link between ballot structure and multipartism hypothesized and rejected by Rae is in fact valid for one major category of electoral systems—single-member district systems. In other words, the proximal effects of electoral systems are much stronger than their distal effects. The theoretical significance of this finding, which I shall discuss in greater detail in the concluding section, is that strategic (or insincere) voting and strategic elite behavior play a considerably less important role in helping the larger, and hurting the smaller, parties than is usually supposed.

How do my methods differ from, and how are they superior to, Rae's? Five differences are of importance throughout the analysis. First in contrast with Rae's treatment of each election as a different case, my unit of analysis is the electoral system, defined as one or more elections held under basically the same or closely similar rules. For instance, instead of counting the 11 Israeli elections as 11 separate cases, I have only two cases: the PR system that used the d'Hondt formula (the 1949 election and the four elections from 1973 to 1984) and the system under the largest-remainders rule (the six elections in the 1951–69 period). My approach avoids the artificial and deceptive inflation of the weight of the available evidence; elections under the same rules are not really independent cases but merely repeated operations of the same electoral system. It also avoids the related problem of artificially weighting the evidence. For instance, France held only 3 elections under the pure d'Hondt rule while the Netherlands had 12; but this is not a good reason to give the French evidence only one-fourth the weight of the Dutch. This approach yields 32 cases (based on 255 elections) in the 1945–85 period.

Second, I use the evidence of all of the available cases as much as possible. For instance, in contrast with Rae, who simply disregards the difficult cases of two-tier systems that use different formulas at

their two levels, I determine which level is of predominant importance and classify the electoral system according to the formula at this level. This procedure increases the number of cases in my analysis from 20 to 31; the only case that I cannot use for the purpose of studying the effects of electoral formulas is France (1951-56), because it used majority, d'Hondt, and largest remainders formulas in such a complex way (in different parts of the country, not at different levels) that they cannot be disentangled.

Third, since Rae finds that both formula and magnitude strongly affect proportionality and multipartism, it is a curious omission in his analysis that he fails to examine the possible interaction of his two explanatory variables. I correct this omission by systematically controlling for the influence of the other independent variable.

Fourth, in order to measure the degree of disproportionality of different electoral systems, I use John Loosemore and Victor J. Hanby's (1971) index *D*. It has become the most widely used index of disproportionality (see, e.g., Mackie and Rose 1982, 411-12; Rose 1984; Taagepera and Shugart 1989, 104-11). *D* is the total percentage by which the overrepresented parties are overrepresented—which is, of course, the same as the total percentage of underrepresentation. In order to calculate *D*, the absolute values of all differences between the parties' vote and seat shares are added and then divided by 2. Rae's measure *I* appears to be similar. It, too, entails the addition of all vote-seat share differences; but this sum is divided not by 2 but by the number of parties. As Richard S. Katz (1980, 140) has pointed out, Rae's index *I* has the grave defect of "giving too much weight to small parties; at the extreme, if the infinite number of (hypothetical) parties that receive no votes and obtain no seats is included, every electoral system would appear

perfectly proportional." Rae (p. 84) tries to avoid this problem by disregarding parties with less than .5 percent of the vote, but this arbitrary cutoff point is still quite low; the presence of several parties with just over .5% of the votes will depress *I* even if these parties fail to win any seats.³

Finally, a relatively minor difference between Rae's methods and mine concerns the measure of multipartism. Rae uses a large number of indicators of multipartism—such as the total number of parties contesting an election, the total number elected to the legislature, the vote and seat shares of the largest party, and the vote and seat shares of the two largest parties combined—but his most important and comprehensive measure is the fractionalization of the party system, which is sensitive to both the numbers of parties and their relative sizes (pp. 47-64, 67-68, 84-85). I shall use an adaptation of Rae's fractionalization index—Laakso and Taagepera's (1979) "effective number of parties"—which carries exactly the same information as Rae's index but is more meaningful. For instance, in a party system with two equally strong parties, the effective number of parties is exactly 2.0; for three equal parties it is 3.0; for two strong parties and one weaker party, it will be somewhere in the neighborhood of 2.5. Rae's fractionalization indexes can be easily converted into the Laakso-Taagepera measure.⁴ The effective number of parties (as well as the index of fractionalization) can be calculated on the basis of either the parties' vote or seat shares. The former is the better indicator of the long-term nature of the party system. It is affected by the operation of the electoral system in previous elections but not yet by the translation of votes into seats in the current election. I shall therefore use the effective number of *elective* (instead of *legislative*) parties as my indicator of multipartism.

Electoral Formulas and Electoral Disproportionality

Rae finds, as expected, that plurality and majority formulas are considerably less proportional than PR but, rather surprisingly, that there is not a great deal of difference within the family of PR systems. The main reason for the latter, unexpected conclusion is Rae's improper classification of the different PR formulas. He hypothesizes that highest averages (or divisor) formulas yield less proportional results than largest remainders (quota) formulas. The distinction between these two types of list PR formulas is indeed an important one—but mainly as regards the practical procedures election officials have to use to allocate seats to party lists. However, the two groups of methods should not be expected to differ with regard to the proportionality they produce, since the degree of proportionality depends on the particular quota that quota methods use and the particular divisor used by divisor methods. This means that differences with regard to proportionality should occur *within* each group instead of *between* them.

Among the highest averages methods, the d'Hondt formula (which uses the divisor series 1, 2, 3, 4, etc.) is the least proportional and systematically favors the larger parties. It contrasts with the Sainte-Laguë formula, which, in the original form proposed by its inventor (using the odd-integer divisor series 1, 3, 5, 7, etc.), approximates proportionality very closely and treats large and small parties in a perfectly evenhanded way. In practice, the Sainte-Laguë method is more often used in a modified form that uses 1.4 instead of 1 as the first divisor, thereby making it harder for small parties to gain their first seats and hence reducing the proportionality of the election result to some extent. According to their logical properties, therefore, the three highest averages methods can be placed on the

following scale from the most to the least proportional: pure Sainte-Laguë, modified Sainte-Laguë, and d'Hondt (see Balinski and Young 1982, 60–66; Lijphart 1986, 172–75).

Similar differences occur within largest remainders (LR) systems. The oldest and best known of these simply uses as its quota the total number of valid votes cast in a district divided by the district magnitude (m , the total number of seats available in the district). This quota, usually referred to as the Hare quota, is impartial as between small and large parties and tends to yield closely proportional results. Less proportional outcomes are produced by the Droop quota, which divides the votes by $m + 1$ instead of m , and the Imperiali quota, which uses $m + 2$ as the denominator.⁵ The use of these lower quotas means that there will be fewer remaining seats to be allocated and hence also more wastage of remaining votes, which is especially harmful to the smaller parties and results in a decrease in proportionality. It can be shown that when the quota is lowered even further, to the extent that there will not be any remaining seats, the outcome becomes exactly the same as that of the d'Hondt formula (Van den Bergh 1955, 68–72). In preferential PR, usually referred to as single transferable vote (STV) systems, the quota is as important for the proportionality of the electoral outcome as in list-PR largest-remainders systems. The one case of STV among the Western democracies—Ireland—uses the Droop quota.

This discussion suggests a much stronger and more plausible hypothesis about the effects of PR formulas on the proportionality of the election result than Rae's hypothesis, which is based on the twofold distinction between largest-remainders and highest-averages methods. Mine is based on a threefold classification: (1) pure Sainte-Laguë and LR-Hare are likely to be the most proportional; (2) modified Sainte-Laguë, LR-Droop, LR-

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Table 1. Average Degrees of Disproportionality of Electoral Systems Classified by Electoral Formula and Adjusted District Magnitude, 1945-85 (%)

Adjusted District Magnitude	LR-Hare and Pure Saint-Laguë	LR-Droop, LR-Imperiali, Modified Sainte-Laguë, and STV-Droop	d'Hondt	Plurality and Majority	All ^a
1-1.1	—	—	—	12.93 (6)	12.93 (6)
1.1-5	—	4.60 (1)	8.51 (3)	—	7.53 (4)
5-10	—	5.18 (3)	5.83 (6)	—	5.61 (9)
10-25	2.81 (2)	—	4.28 (3)	—	3.69 (5)
100-150	2.46 (3)	3.53 (2)	4.39 (2)	—	3.32 (7)
All ^a	2.60 (5)	4.53 (6)	5.87 (14)	12.93 (6)	6.45 (31)

Source: Based on data in Mackie and Rose 1982, 1983, 1984; Mackie 1985, 1986; Ornstein, Mann, and Malbin 1987, 47-48; and Luxembourg, Service Central de la Statistique 1984, 120-21.

Note: The numbers of cases on which the percentages are based are in parentheses.

^aExcept France of 1951-56.

Imperiali, and STV-Droop should be less proportional; and (3) d'Hondt is likely to be the least proportional of the PR formulas.

The bottom row of Table 1 shows that this hypothesis is strongly supported by the evidence. The plurality and majority systems are more than twice as disproportional as d'Hondt PR, and the d'Hondt systems are more than twice as disproportional as the LR-Hare and pure Sainte-Laguë systems; the in-between PR systems are also, as hypothesized, roughly intermediate in terms of their degree of disproportionality. These figures are based on 31 of the 32 cases, as explained earlier.⁶ My findings partly support and partly contrast with Rae's. Rae (pp. 96, 106) finds a similar difference between plurality-majority and the least proportional PR systems: 3.96% and 1.78% respectively (measured in terms of Rae's *I*). But the difference between his two types of PR is quite small: 1.22% and 1.78% (also measured in terms of *I*).

Given the weakness of Rae's hypothesis about PR systems, how can we account for the fact that he finds *any* difference

between his two types? After carefully checking all of the likely explanations, I found that neither Rae's different methodology nor his empirical errors (of the 56 list PR elections that according to Rae's own criteria, should have been included in his analysis, 28 are misclassified or omitted) provide the answer. The explanation is that d'Hondt happens to be the most frequently used highest average formula: of the 18 highest averages cases, 14 are d'Hondt. Since d'Hondt is also the least proportional of all PR methods, it is now clear why Rae's category of highest averages turns out to be less proportional than that of his largest remainders. His finding, modest as it is, is entirely fortuitous.

Table 1 also shows the indices of disproportionality of the PR formulas when district magnitude is held constant. Within each category of adjusted magnitude (to be defined in the next section), the differences between the formulas are reduced to some extent; but they still show up clearly and they are all in the expected direction.

District Magnitude and Electoral Disproportionality

The strongest relationship found by Rae is that between district magnitude and proportionality: as magnitude increases, disproportionality goes down. He compares the effects of electoral systems classified according to five categories of average district magnitude, m : (1) single-member districts where $m = 1$, (2) m between 2 and 6, (3) m between 6 and 10, (4) m between 10 and 20, and (5) m between 100 and 150, which occurs in the two countries with a single nationwide district, Israel and the Netherlands. Rae never explains why he selected these particular five categories; but with one exception the dividing lines make good sense, since they coincide with natural breaks in the distribution of the magnitudes. The exception is that the natural discontinuity in the range between single-member districts and a district magnitude of 10 occurs at about $m = 5$ instead of $m = 6$. Another small improvement is to make 1.1 the dividing point between the two lowest categories. This takes care of the few deviations from single-member districts in the plurality and majority countries, which are all essentially single-member district systems; and it removes Rae's awkward gap between $m = 1$ and $m = 2$. A final helpful adjustment is to expand the 10-to-20-seat category to 10-to-25 so that it can accommodate the Austrian case, which since 1971 has had a magnitude of slightly higher than 20.⁷

Average district magnitude is defined as the average number of seats (or representatives) per district; and it can be calculated easily by dividing the total number of seats in the legislature by the number of districts. Only with regard to two-tier districting systems do we run into complications. The solution proposed by Rae (p. 21) is that "this problem is tentatively resolved by counting the total number of districts at both levels in the computation

of average magnitudes." This is an unsatisfactory solution because its effect is to make the average magnitude lower than it would be on the basis of either the low-tier or the high-tier districts by themselves. For instance, from 1945 to 1970 the Austrian 165-member legislature was elected in 25 low-tier and 4 high-tier districts. Counting the lower tier only, the average magnitude would be 6.6; counting the higher tier only, it would be 41.2. The most reasonable assumption is that the "true" magnitude should be somewhere between 6.6 and 41.2. According to Rae's definition, however, it is 5.7—well below 6.6. Rae states that his solution is only tentative, but he never reconsiders it. Since I do not want to prejudge the effect of the superimposition of a higher tier of districts (which is often a single national district), my own tentative and temporary solution is to calculate average magnitude solely on the basis of the lower tier.

Rae's (p. 124) hypothesis concerning the impact of two-tier or "complex" districting is eminently plausible: "One would expect complex districting—the use of two tiers of districts—to increase proportionality at any given level of magnitude." However, Rae immediately rejects this hypothesis. Without supplying precise figures, he states that "the four systems based on complex districting—Icelandic, German, Austrian, and Danish—do not behave in accordance with this expectation. For two of them, the German and the Danish, disproportions are below average for their respective levels of district magnitude. But for the Austrian and Icelandic systems, complex districting associates itself with greater-than-average disproportions, given their levels of district magnitude." It is worth noting that Rae's conclusion, for once, is based on a controlled test. He does not simply compare all complex-districting cases with all simple-districting cases but controls for district magnitude.

Nevertheless, Rae's negative finding is

unsatisfactory for several reasons. One is that he fails to recognize and include Belgium and Italy as complex-districting cases. Second, he suddenly shifts from elections to countries as his units of analysis and thereby ignores the major electoral law changes in Iceland in 1959 and in Germany after the 1953 election. Third, his conclusion is so surprising that a closer look at the deviant cases is called for. Rae (p. 124) gives up too quickly: "Since no plausible explanation is available . . . , it must be concluded that complex districting does not produce greater proportionality than simple districting at any given level of district magnitude." In fact, the deviant Austrian and Icelandic cases do have some special features that may explain their unexpected disproportionality. In the Icelandic electoral system from 1946 to 1959, more than half of the seats at the lower tier were in single-member districts and almost a third in two-member districts. The few adjustment seats available at the national level could not neutralize the disproportionalities caused by these low magnitudes. The Austrian electoral system from 1945 to 1970, despite the country's relatively small size, had four higher-tier districts instead of one national district.

The Austrian case suggests a general explanation. Complex-districting systems typically try to restrict the benefits obtainable at the higher tier to parties that have received a minimum number of votes or seats at the lower tier. Since this entails discrimination against the smallest parties, it also places a limit on the proportionalizing effect of two-tier districting systems; and it means that the higher-tier districts must be large, preferably nationwide, in order to make the election result appreciably more proportional. A stronger hypothesis than Rae's would therefore be that complex districting with nationwide—or almost nationwide—higher-tier districts (which can be called *strong complex districting*) yields greater proportionality than other forms of com-

plex districting or simple districting. The one case of near-nationwide districts is Austria since 1971, which has used only two higher-tier districts. For the reasons just outlined, the 1946–59 Icelandic electoral system should not be included among the strong complex-districting cases. When the disproportionalities of the two types of districting systems are contrasted, the strong complex-districting systems do indeed turn out to be more proportional within each category of magnitude than the other systems.

I can now propose a modification in my tentative definition of average district magnitude: strong complex-districting systems should be classified one category higher than what their lower-tier magnitude would entitle them to. I shall call this their *adjusted* district magnitude. For instance, since 1970 Sweden has had a lower-tier magnitude of 12.5 seats but also a single, nationwide, higher tier; its adjusted magnitude is therefore in the 100-to-150-seat category together with Israel and the Netherlands. The relationship between adjusted magnitude and disproportionality is considerably stronger than that between magnitude, as I tentatively defined it, and disproportionality. The last column of Table 1 presents the average degrees of disproportionality in the five classes of adjusted magnitude. The greatest difference again occurs between the plurality-majority systems (which are also the single-member district systems) and the least-proportional category of PR systems. Within the family of PR systems, the differences among the different categories of magnitude are roughly similar to the differences found earlier among the several formulas. These findings are in sharp contrast with Rae's (pp. 114–24) conclusion that district magnitude is a more potent force than the electoral formula, which has become the conventional wisdom (Sartori 1986, 53, 66; see also Taagepera and Shugart 1989, 112–25).

What is the effect of each of the inde-

pendent variables—electoral formula and district magnitude—on the dependent variable of proportionality when the other independent variable is controlled? This question is relevant only for the PR systems since the non-PR systems do not vary with regard to magnitude. Table 1 also shows the indices of disproportionality of all PR cases classified according to both effective district magnitude and electoral formula. When district magnitude is held constant, the differences between the formulas are reduced but not at all eliminated. When we control for electoral formula, sizable differences between the magnitude classes remain in the d'Hondt systems; but the differences are less impressive for the other formulas. The reason why these relationships now appear less strong is that effective district magnitude and electoral formula are themselves correlated. As the numbers in parentheses in Table 1 show, the most proportional formulas (LR-Hare and pure Sainte-Laguë) occur only in the two most proportional classes of adjusted magnitude (those larger than 10 seats); and the majority of the least-proportional formulas (d'Hondt) are used in the least-proportional magnitude groups (less than 10 seats).

Because Table 1 has several empty cells, its evidence is somewhat ambiguous. Moreover, there are a couple of cells with values that are out of monotonic order. Nevertheless, the overall pattern is one of interaction between formula and magnitude but also of the independent impact of each of them on the proportionality of the electoral outcome. Further evidence of this pattern is provided by Figure 1, which shows the changes in disproportionality due to major electoral law changes *in the same country*. For instance, Sweden first shifted from d'Hondt to modified Sainte-Laguë and subsequently to a much higher adjusted magnitude. Because country-specific characteristics are now also controlled, we have a rigorous test of the ef-

fect of magnitude and formula on disproportionality. The nine major electoral law changes *all* produce the expected changes in the degree of disproportionality. With the exception of Israel's return from LR-Hare to d'Hondt after the 1969 election, all of these major changes entailed shifts to more proportional rules.

To sum up, district magnitude was found to have a very strong effect on proportionality, confirming Rae's conclusion. This effect remains strong—albeit not *as* strong—when the electoral formula is held constant, a control that Rae fails to apply. And contrary to Rae, complex districting was found to make the election result considerably more proportional.

Formula, Magnitude, and Multipartism

The discussion of the effect of the electoral formula and district magnitude on proportionality has paved the way for a more expeditious treatment of their effect on multipartism. The reason is that proportionality is the logical link that connects these electoral system variables to the number of parties: the more proportional the electoral system is, the more favorable it is for small and new parties and hence the more it will allow—some would say *encourage*—a large number of parties. Of course, there are also other important causes of multipartism, particularly the number and depth of the cleavages in a society (Taagepera and Grofman 1985). We should therefore not expect a very strong correlation between magnitude and formula on the one hand and multipartism on the other. Rae (p. 141) correctly points out that "party systems are influenced by many variables—social, economic, legal, and political. [The] electoral law . . . is to be counted only one of many determining forces. And it is, secondly, impossible to sort out all the contributing factors, or to assign even ap-

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Figure 1. Average Disproportionality and Average Number of Elective Parties of Electoral Systems in Change, 1945-85

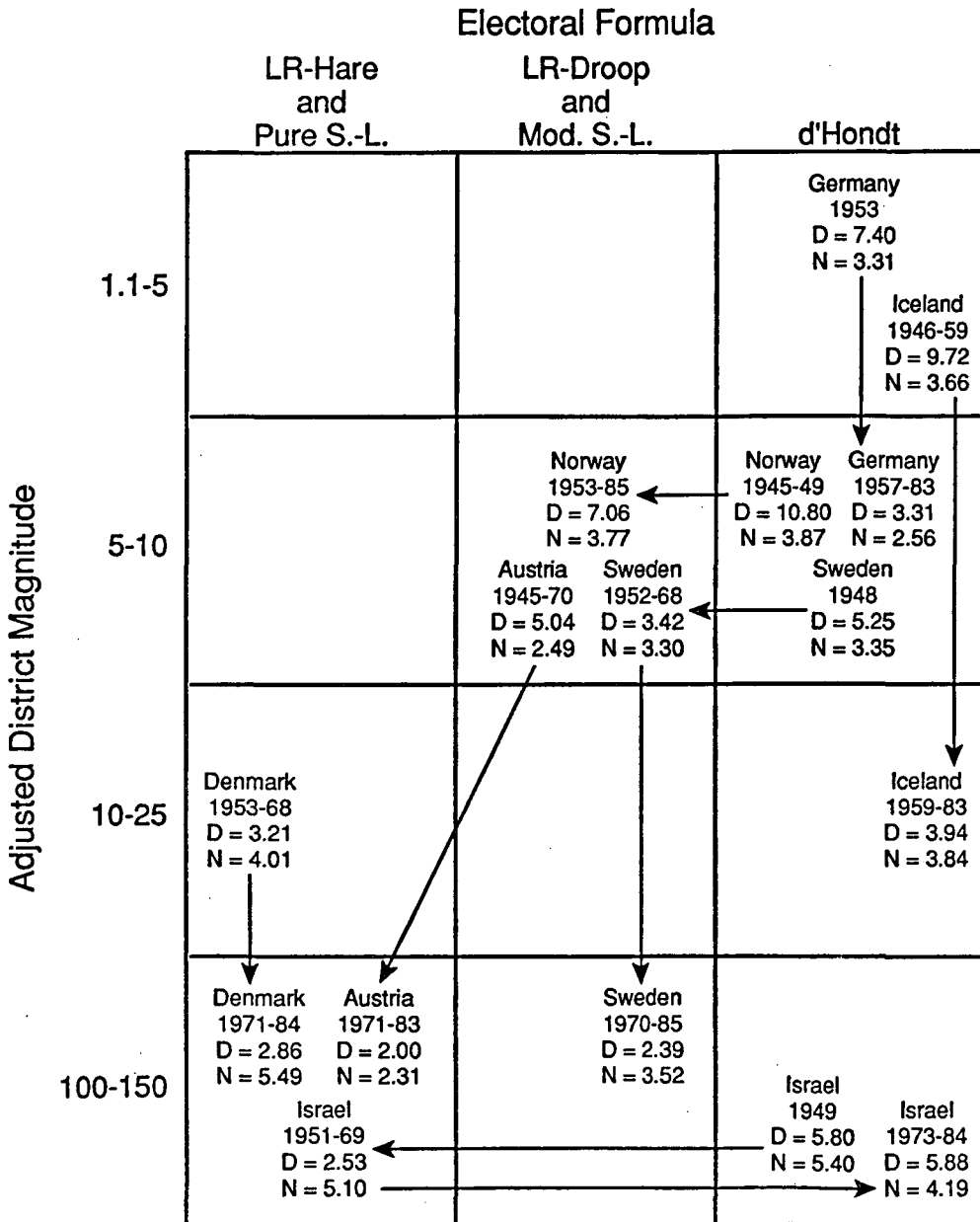


Table 2. Average Effective Numbers of Elective Parties in Electoral Systems Classified by Electoral Formula and Adjusted District Magnitude, 1945-85

Adjusted District Magnitude	LR-Hare and Pure Sainte-Laguë	LR-Droop, LR-Imperiali, Modified Sainte-Laguë, and STV-Droop	d'Hondt	Plurality and Majority	All ^a
1-1.1	—	—	—	2.95 (6)	2.95 (6)
1.1-5	—	3.04 (1)	3.96 (3)	—	3.73 (4)
5-10	—	3.19 (3)	4.10 (6)	—	3.80 (9)
10-25	4.04 (2)	—	4.27 (3)	—	4.18 (5)
100-150	4.30 (3)	3.75 (2)	4.76 (2)	—	4.28 (7)
All ^a	4.20 (5)	3.35 (6)	4.20 (14)	2.95 (6)	3.79 (31)

Source: Based on data in Mackie and Rose 1982, 1983, 1984; Mackie 1985, 1986; Ornstein, Mann, and Malbin 1987, 47-48; and Luxembourg, Service Central de la Statistique 1984, 120-21.

Note: The numbers of cases on which the percentages are based are in parentheses.

^aExcept France of 1951-56.

proximate weights to them. Worse yet, electoral laws are themselves shaped by party systems."

Notwithstanding these cautionary words, Rae (p. 98) reports a big difference in multipartism for plurality and majority systems (which are also the single-member district systems) on the one hand and PR multimember district systems on the other—2.17 and 3.70 parties respectively. My figures show more modest differences—2.95 and 4.07 parties. The main reason why Rae finds such a small effective number of parties in plurality and majority systems appears to be that he counts in terms of elections: the 2 French Fifth Republic multipart party elections (5.56 parties) are overwhelmed by the 10 almost purely two-party U.S. elections (2.04 parties). On the other hand, if we focus on the plurality systems—Canada, New Zealand, the United Kingdom, and the United States—and exclude the French and Australian majority systems, the degree of multipartism is considerably lower—only 2.54 parties.

Since the differences in disproportionality among PR systems are smaller than those between PR and plurality-majority, we should also expect more modest differ-

ences with regard to multipartism. Rae (p. 106) reports that in accordance with his hypothesis, highest-average formulas are associated with lower multipartism than largest-remainder formulas—3.57 and 4.00 parties respectively. However, when the missing 1945-64 data and the 1965-85 data are added and the numerous misclassifications are corrected, the difference disappears—4.39 and 4.40 parties.

My own findings are hardly more impressive, however. They are summarized in Table 2. The d'Hondt systems have exactly as many parties as the most proportional LR-Hare and pure Sainte-Laguë systems (see the bottom row of the table). Also unexpectedly, the intermediate systems in terms of proportionality (LR-Droop, etc.) are considerably lower than the d'Hondt systems with regard to multipartism. We must reject the hypothesis that within the PR family, formula and multipartism are related.

The findings concerning district magnitude are somewhat more positive (see the last column of Table 2) but not as strong as Rae (p. 121) suggests. Rae reports a monotonic relationship between magnitude and the number of parties for the PR systems—from 3.18 parties in the lowest

category of magnitude to 4.65 parties in the highest, an increase of approximately 1.5 parties. My reanalysis shows a much smaller increase—only about .5 parties.

When the two independent variables are cross-tabulated, the weak relationship between magnitude and multipartism is not weakened further. The monotonic increase in multipartism as magnitude goes up is even maintained within each of the three groups of formulas, but the differences are small. Controlling for magnitude makes the influence of the electoral formula on multipartism look worse. For each magnitude class, multipartism is lower instead of higher in the intermediate category than in d'Hondt systems. And in two of the four classes of magnitude that allow a comparison of d'Hondt with LR-Hare and pure Sainte-Laguë, multipartism is higher, not lower, in the d'Hondt systems—again contrary to our expectations based on the formulas' tendencies to disproportionality.

Similar, mainly negative, evidence is provided by Figure 1, which applies the within-country test of the effect of major electoral law changes—applied earlier to changes in disproportionality—to changes in multipartism. Whereas the electoral law changes produced the expected changes in disproportionality in all nine cases, there is no commensurate pattern for multipartism: the evidence runs five to four *against* the hypothesis. Of the five changes in electoral formula, only one yields the expected change in the degree of multipartism; and of the five shifts to larger magnitudes (one case, Austria, entails a change in both formula and magnitude) three yield more, and two yield less, multipartism.

These findings suggest that the impact of disproportionality on the number of parties is even weaker than our modest expectations had led us to believe. When we take a direct look at this relationship, we find that it is in the expected direction but that the correlation coefficient is an

almost negligible $-.10$. However, as I shall point out shortly, this negative finding needs to be qualified in several respects.

Ballot Structure and Multipartism

With regard to the impact of ballot structure on multipartism, Rae formulates his most original hypothesis. Based on the distinction between ordinal ballots, which "allow the voter to favor more than one party with his mandate," and categorical ballots, which "require that the voter give his mandate to a single party," Rae (p. 126) hypothesizes that ordinal ballots "allow each voter's mandate to be dispersed among several parties, thereby producing a sort of microfractionalization" and that elections held with such ballots, and hence with repeated microfractionalizations, "produce more fractionalized elective party systems than would be found under other elections." However, he finds that the relationship is the other way around: his ordinal systems have an average effective number of only 2.94 parties compared with 3.23 parties in categorical systems. His conclusion is, "My theory is absolutely wrong" (p. 127).

Here again, we must take a critical look at Rae's methods and procedures. He makes two serious mistakes of classification. His ordinal ballot systems are Australia, Ireland, Luxembourg, and Switzerland. The first two use preferential ballots that allow—in the Australian case, *require*—the voters to rank-order candidates and parties according to the voters' preferences. Luxembourg and Switzerland use list PR systems but give each voter as many votes as there are seats in the district and allow the voter to distribute these votes over two or more parties, equally or preferentially. These four electoral systems are correctly classified. However, Rae misclassifies the German and French Fifth Republic systems as cate-

**Table 3. Average Effective Numbers of Elective Parties Classified by
Adjusted District Magnitude and Ballot Structure, 1945-85**

Ballot Structure	Adjusted District Magnitude					All
	1-1.1	1.1-5	5-10	10-25	100-150	
Categorical	2.54 (4)	4.28 (2)	3.99 (8)	4.34 (4)	4.28 (7)	3.92 (25)
Ordinal	3.78 (2)	3.18 (2)	4.00 (2)	3.53 (1)	—	3.64 (7)
All	2.95 (6)	3.73 (4)	3.99 (10)	4.18 (5)	4.28 (7)	3.85 (32)

Source: Based on data in Mackie and Rose 1982, 1983, 1984; Mackie 1985, 1986; Ornstein, Mann, and Malbin 1987, 47-48; and Luxembourg, Service Central de la Statistique 1984, 120-21.

Note: The numbers of cases on which the percentages are based are in parentheses.

gorical; they are clearly ordinal. The French two-ballot system allows the voters to vote for different parties on the first and second ballots and quite often—when candidates are eliminated or withdraw after the first ballot—*require* that they do so. In Germany, each voter has two votes, which may be cast for different parties; this ordinal ballot system has existed since the 1953 election.⁸

When these misclassifications are corrected and also, as usual, the missing data are added and electoral systems rather than elections are used as the cases for analysis, the findings are still negative (see the last column of Table 3). The ordinal systems have less, instead of more, multipartism than categorical systems; although the differences are relatively small. In trying to explain his negative result, Rae (p. 129) speculates that "ballot structure constitutes a rather weak variable. It may be much less important than electoral formulae, district magnitude, and, more likely still, the sociopolitical forces which underlie party competition." He continues, "Unfortunately, the available data allow no definitive test of this notion, and it must be left unverified." This is an unnecessary defeatist stance, since we can test the influence of ballot structure on multipartism *while controlling for formula and magnitude*.

Table 3 shows the average effective

numbers of parties under categorical and ordinal ballots when magnitude is controlled. In the multimember district systems, my findings are still mainly negative: two of the three pairs of values still run counter to the hypothesis, and the one positive instance is only marginally positive. However, in single-member district systems, ordinal ballots result in a dramatically higher degree of multipartism than categorical ballots. The difference is roughly that between a two-and-a-half-party system and an almost-four-party-system. The explanation is not difficult to find. The categorical systems are plurality systems in which the incentive to vote strategically or insincerely, in order not to waste one's vote, is strong. This results in greater support for the large parties by voters and by politicians and a reduction in the effective number of parties. The ordinal ballot removes most of the risk of wasting one's vote; hence it encourages both sincere voting and multipartism.

When we control for the electoral formula, we find the same important difference among the plurality-majority formulas, since these coincide with the single-member district systems, and the same mainly negative result for the PR systems.⁹ The link between ballot structure and multipartism applies only to single-member district systems, but it is a strong link.

Conclusion

The two electoral system variables, formula and magnitude, have a strong effect on electoral disproportionality (considerably stronger than reported by Rae) but only a weak effect on multipartism (much weaker than Rae suggests). The reason is that disproportionality—the intervening variable between the electoral system and multipartism—is only a weak predictor of the number of parties. However, this conclusion has to be qualified in three important respects.

In the first place, the hypothesis that disproportionality should reduce multipartism is based on the assumption of strategic behavior. When smaller parties are expected to be discriminated against, voters, as well as politicians, political activists, and money givers will favor the larger parties (Gunther 1989; Riker 1986, 33–41). The finding on the effect of ballot structure suggests that the majority systems should be excluded from the hypothesized relationship. They are characterized by high disproportionality; but because of their “ordinal” ballot structure, they do not encourage strategic behavior and hence do not discourage multipartism.¹⁰ The case of France of 1951–56 should also be excluded because its disproportionality was designed to favor the smaller parties—those in the ideological center—and therefore encouraged instead of discouraged multipartism (Campbell 1965, 113–26). For the remaining 29 cases, the coefficient of correlation between disproportionality and the effective number of parties is now $-.29$ (compared with an r of $-.10$ for all 32 cases).

The second qualification is that this somewhat stronger relationship mainly reflects the differences between the four plurality systems with high average disproportionality (11.08%) and low average multipartism (2.54 parties) on the one hand and the 25 PR systems with lower disproportionality (4.89%) and higher

multipartism (4.00 parties) on the other. However, within the PR category, there is virtually no relationship between the two variables; the correlation is $.02$. This finding casts doubt on Sartori's (1986, 54) assertion that within the family of PR systems, a distinction must be made between highly proportional, hence “feeble,” systems and less proportional “strong-feeble” systems, with the latter in an intermediate position between the “feeble” systems and “strong” plurality systems.

The third qualification is that since the focus has been on gauging the electoral system's long-range effect on the party system, multipartism has been consistently measured in terms of the effective number of *elective* parties. It is logical to expect a stronger impact of disproportionality on the effective number of *legislative* parties because the latter is affected not only by strategic responses to the expectation of discrimination against smaller parties—what Rae (pp. 67–68) calls the electoral system's “distal” effect and Maurice Duverger (1963, 226) its “psychological” effect—but also by the immediate, “proximal” (Rae) or “mechanical” (Duverger) effect of the translation of votes into seats in the particular election under observation. For the 29 cases, the correlation between disproportionality and the effective number of legislative parties is a stronger $-.45$. This means that disproportionality explains only about 8% of the variance in elective multipartism but about 20% of that in legislative multipartism. The latter percentage must be interpreted as the combined effect of the psychological and mechanical factors, but it still indicates that the mechanical factors are the stronger influences.¹¹

To sum up, electoral systems display wide and predictable differences in disproportionality, smaller differences in multipartism that are the direct effect of disproportionality, and even smaller differences in elective multipartism due to strategic calculations by elites and voters.

Plurality systems are an exception: their high disproportionality accounts, via the effect of strategic behavior, for their relatively small effective number of elective parties. Strategic voting is neutralized in majority systems, which can therefore combine high disproportionality with high elective multipartism. And the substantial differences in disproportionality among PR systems are apparently not large enough to produce either commensurate differences in strategic behavior or, as a result, commensurate differences in elective multipartism.

Notes

This is a revised and much shortened version of a paper presented at the fourteenth World Congress of the International Political Science Association (IPSA), Washington, 1988. I should like to acknowledge the assistance and advice, especially with regard to electoral laws and election data, that I have received from my collaborators in the comparative and collaborative project of which this is a product: Don Aitkin (Australian Research Council, Canberra), Asher Arian (Tel Aviv University, Israel), Thomas C. Bruneau (Naval Postgraduate School, Monterey), Ivor Crewe (University of Essex), Wilfried Dewachter (Catholic University of Leuven, Belgium), A.-P. Frogner (Catholic University of Louvain, Belgium), William P. Irvine (Queen's University, Kingston, Canada), W. K. Jackson (University of Canterbury, Christchurch, New Zealand), Gary C. Jacobson (University of California, San Diego), Markku Laakso (University of Kuopio, Finland), Rafael López Pintor (Autonomous University, Madrid), Thomas T. Mackie (University of Strathclyde, Glasgow), George Th. Mavrogordatos (University of Athens), Sten S. Nilson (University of Oslo), Dieter Nohlen (University of Heidelberg), Cornelius O'Leary (Queen's University, Belfast), Jea-Luc Parodi (Fondation Nationale des Sciences Politiques, Paris), Mogens Pedersen (Odense University, Denmark), Anton Pelinka (University of Innsbruck), Bo Särilvik (Gothenburg University, Sweden), Yasunori Sone (Keio University, Tokyo), Alberto Spreafico (University of Florence), Björn S. Stefánsson (Agricultural Research Institute, Reykjavik), Jürg Steiner (University of North Carolina, Chapel Hill), and Jan Verhoef (Ministry of Housing, Physical Planning, and the Environment, The Hague). I am also grateful to the IPSA panel participants and to André Blais, Keith Bybee, David Collier, Richard Gunther,

Richard G. Niemi, Matthew S. Shugart, and Rein Taagepera for helpful comments. And I should like to express my appreciation for the financial support of the Guggenheim Foundation and the German Marshall Fund of the United States.

1. My references will be to the 1971 edition of Rae's book.

2. Rae was so kind as to make his original data for 1945-64 available to me, but I decided that it was preferable to use the election data in Thomas T. Mackie and Richard Rose's (1982) standard handbook in order to facilitate replication by other researchers and because I also used the latter data for the 1965-85 period. The Mackie-Rose handbook includes the elections held through 1981. For the 1982-85 elections I turned to the annual updates in the *European Journal of Political Research* by Mackie and Rose (1983, 1984) and Mackie (1985, 1986). The results of U.S. House of Representatives elections, not reported by Mackie and Rose, were taken from Ornstein, Mann, and Malbin 1987. Since the Luxembourg voting figures provided by Mackie and Rose fail to adjust for the unequal numbers of votes that voters have in different districts, I used Luxembourg, Service Central de la Statistique 1984.

3. It may be argued that compared with *I*, *D* errs in the opposite direction: it has a slight tendency to exaggerate the disproportionality of systems with many parties. However, the two-major-parties index (defined as the average vote-seat share deviation of the two largest parties), which was expressly designed to steer a middle course between *D* and *I*, is in practice much closer to *D* than to *I* (Lijphart 1985, 10-12). A great advantage of *D* compared with both *I* and the two-major-parties index is that it does not entail arbitrary decisions either on cutoff points (for *I*, should the cutoff point be .5%, which is Rae's criterion, or should it be 1, 2, 5, or 10%?) or on the number of parties to be counted. (Instead of the two largest parties, why not the three largest parties or perhaps just the largest party?) Mackie and Rose (1982) and Rose (1984) use a slightly different form of *D*. Instead of using the "negative" index of disproportionality, they prefer to think more positively in terms of an index of *proportionality*; that is, instead of *D*, they subtract *D* from 100%.

4. If the index of fractionalization is represented by *F* and the effective number of parties by *N*, they are related as follows: $N = 1/(1 - F)$. The *F* values corresponding to 2.0, 2.5, and 3.0 parties are .50, .60, and .67.

5. In Continental Europe, the Droop quota is often called the Hagenbach-Bischoff quota. Strictly speaking, there is a slight difference between the two. If *v* stands for the total number of district votes and *m* for the district magnitude, the Hagenbach-Bischoff quota is $v/(m + 1)$, usually rounded up, whereas the Droop quota is defined as $v/(m + 1) + 1$, rounded down if necessary (Leonard and Natkiel 1987, 3). Therefore, the two quotas differ if $v/(m +$

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1) happens to be an integer; but in mass elections where v is measured in thousands of votes, this difference is so tiny that it can be safely ignored. To make matters even more complicated and (unfortunately) confusing, the term *Hagenbach-Bischoff method* is sometimes also used to describe a divisor system, as in Switzerland. This method merely represents a shortcut in the application of the d'Hondt formula, and its results are exactly the same as those of d'Hondt. For all practical purposes, therefore, the two can be regarded as identical. Finally, I have to add a qualification to my description of the Imperiali quota, which is used exclusively in Italy. It has been defined as $v/(m + 2)$ since the 1958 election; but it was even lower— $v/(m + 3)$ —in the 1948 and 1953 elections and, on average, slightly higher in 1946, when $v/(m + 2)$ applied to the higher-magnitude districts and $v/(m + 1)$ (in effect, the same as the Droop quota) in the smaller districts (Carstairs 1980, 157–59; Spreafico 1983, 188–90, 194).

6. In order to classify the difficult two-tier systems according to electoral formula, I distinguish between two basic types. In one type the districts at the lower level are used for the initial allocation of the seats; but the final allocation takes place at the higher level, often the national level, on the basis of all of the votes cast in all of the lower-tier districts that together make up the higher-level district. Thus, the formula at the higher level is clearly the more important one. Belgium, Denmark, Germany, and Iceland (1946–59) fit this type. (Sweden from 1970 on is another example; but since it uses the same formula at both levels, it does not represent a classification problem. This also applies to Iceland since 1959, but not to the earlier case of Iceland). The second type uses one of the varieties of largest-remainders formulas in the lower-tier districts; but instead of allocating the remaining seats to the remaining votes in these districts, all remaining votes and seats are transferred to and allocated in higher-tier districts. Here the formula at the lower level is decisive. No higher-level formula is able to favor the larger over the smaller parties systematically, since the parties with the highest totals of remaining votes are not necessarily the largest parties. Rather, the lower-tier formula determines how many seats will be available at the higher level, which is of crucial importance to the smaller parties. Only LR-Hare at the lower tier is fully proportional. Examples of the second type are Austria and Italy. Because some of these six complex-districting countries underwent changes in formula, district magnitude, or ballot structure, there are 11 additional cases of electoral systems with which the hypothesis can be tested.

7. An unintended but welcome byproduct of these adjustments is that the few erroneous magnitude values supplied by Rae no longer entail misclassifications.

8. I treat the 1953 election and the elections from

1957 on as two separate cases because they differ with regard to district magnitude.

9. In spite of these negative findings for PR systems, Rae's hypothesis still has a good deal of plausibility, and perhaps it should not be rejected completely. For instance, it seems quite clear that the small but crucially important Free Democratic party (FDP) in West Germany has benefited substantially from its appeal to the voters to give it their second votes (which are actually the more important of the two votes); the FDP has in fact been called (somewhat sarcastically) the "party of second choice" (Dittberner 1987). It is virtually certain that without the ordinal ballot, the party would not have fared so well and that multipartism would have been reduced.

10. This is especially clear in the case of the French Fifth Republic. The Australian ordinal majority system does not behave very differently from the plurality systems.

11. For the 25 PR systems (excluding France of 1951–56), the correlation between disproportionality and legislative multipartism is still only a very weak $- .17$.

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ALTERING THE FOUNDATIONS OF SUPPORT FOR THE PRESIDENT THROUGH PRIMING

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The disclosure that high officials within the Reagan administration had covertly diverted to the Nicaraguan Contras funds obtained from the secret sale of weapons to Iran provides us with a splendid opportunity to examine how the foundations of popular support shift when dramatic events occur. According to our theory of priming, the more attention media pay to a particular domain—the more the public is primed with it—the more citizens will incorporate what they know about that domain into their overall judgment of the president. Data from the 1986 National Election Study confirm that intervention in Central America loomed larger in the public's assessment of President Reagan's performance after the Iran-Contra disclosure than before. Priming was most pronounced for aspects of public opinion most directly implicated by the news coverage, more apparent in political notices' judgments than political experts', and stronger in the evaluations of Reagan's overall performance than in assessments of his character.

Presidents who are popular in the country tend to have their way in Washington. Popularity is a vital political resource, perhaps the president's single most important base of power (Neustadt 1960; Ostrom and Simon 1985; Rivers and Rose 1985). Popularity, in turn, depends on the prevailing economic, social, and political conditions of the times. Unemployment, inflation, economic growth, flagrant violations of public trust, the human toll of war, sharply focused international crises, dramatic displays of presidential authority—all affect the president's standing with the public at large (Hibbs, Rivers, and Vasilatos 1982a, 1982b; Kernell 1978; MacKuen 1983; Ostrom and Simon 1985). A president's popularity (and therefore his power) is shaped by large events

played on a national and international stage.

Our purpose here is to illuminate in greater detail the foundations of public support for the U.S. president by taking an approach different from, but complementary to, the one that now dominates research. In the dominant approach, time series statistical methods are applied to aggregated public opinion data. The typical model includes a handful of macroeconomic measures, an indicator or two to reflect the costs of war, and a miscellaneous set of measures to stand for crises, scandals, domestic unrest, presidential initiatives, and more. Although this approach can point with authority to the important national and international events that drive change in popular support in the aggregate, it cannot tell us about the

dynamics of *individual* change. The aggregate time series results may tell us that scandal weakens the president's support with the public but not how scandal affects the thinking of individual citizens. Other than making the analytically convenient but highly unrealistic claim of homogeneity (that all citizens react in exactly the same way), the time series work is silent on what, exactly, citizens are doing. It does not and cannot tell us what is happening at the individual level.¹

Our approach is to examine processes of change in popular support for a president at the level of the individual citizen, with the goal of informing and enriching aggregate studies of presidential popularity. We pursue this ambition by looking closely at citizens' responses to a single event—the highly publicized and dramatic revelation, on 25 November 1986, that funds received by the United States from the sale of arms to Iran had been secretly channeled by members of President Reagan's National Security Council to the Nicaraguan Contras. We treat the Iran-Contra revelation as a critical test for a theory—which we call *priming*—that claims to provide a comprehensive and psychologically plausible account of how citizens formulate and revise their views of presidential performance.

The Iran-Contra Connection

The Iran-Contra drama began to unfold on 3 November 1986, when a Lebanese magazine reported that Robert C. McFarlane, the President's National Security Advisor, had secretly visited Tehran and that the United States had subsequently sent arms to Iran. In the face of mounting pressure from the news media, President Reagan went public on 13 November, disclosing that a "diplomatic initiative" with Iran had in fact been underway for some 18 months. The purpose of the initiative, he said, was to forge a new relationship

with Iran, to bring an honorable end to the Iran-Iraq war, to eliminate state-sponsored terrorism, and to secure the safe return of the U.S. hostages held in Lebanon. Reagan went on to say that as part of this diplomatic initiative, he had authorized "the transfer of small amounts of defensive weapons and spare parts." He assailed the "wildly speculative false stories about arms for hostages and alleged ransom payments" and concluded with the emphatic declaration, "We did not—repeat, did not—trade weapons or anything else for hostages nor will we," a claim he repeated in a nationally televised news conference on 19 November.

On 25 November, the focus of the brewing scandal shifted abruptly away from Iran and the arms-for-hostages question. At noon that day, Attorney General Meese announced to a national television audience that funds obtained from the secret sale of weapons to Iran had been channeled to the Contras fighting to overthrow the Sandinista government in Nicaragua. The diversion of funds had been accomplished through a covert operation managed by members of the president's National Security Council (NSC). President Reagan then disclosed that Vice Admiral John Poindexter, director of the NSC, had resigned and that staff member Oliver North had been dismissed.

These remarkable revelations immediately took over the national news: suddenly, and dramatically, Nicaragua and aid to the Contras were the focus of front-page stories (see Figure 1).² Such news was not good for President Reagan's popularity. Figure 2 presents results from polls conducted by Gallup, ABC with the *Washington Post*, and CBS with the *New York Times*. All three register sharp declines in public support for President Reagan's performance, roughly coincident with the Iran-Contra revelation. It is impossible to estimate from these data how much of the decline in Reagan's popularity should be traced directly to

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disclosure of the Iran-Contra connection alone, but it is clear that the events of November significantly shook citizens' confidence in their president.

A Theory of Priming

Equipped with our theory of priming, we believe that Reagan's declining popularity can be explained, in part, by the conjunction of two facts: (1) the media's newfound fascination with covert aid to the Contras and (2) the public's opposition to intervention in Central America. According to the priming theory, when faced with a judgment or choice, people ordinarily do not take all plausible considerations into account, carefully examine and weigh all their implications,

and then integrate them all into a summary decision. People typically forgo such exhaustive analysis and instead employ intuitive shortcuts and simple rules of thumb (Kahneman, Slovic, and Tversky 1982). One such heuristic is to rely upon information that is most *accessible* in memory, information that comes to mind spontaneously and effortlessly when a judgment must be made (Fischhoff, Slovic, and Lichtenstein 1980; Higgins and King 1981; Taylor 1982; Tversky and Kahneman 1981). When asked to evaluate a president's performance, U.S. citizens generally focus only on the aspects of their knowledge that happen to be most accessible at the time of judgment.

In turn, what information is accessible for presidential evaluations is determined

Figure 1. Average Number of Lines per Day Devoted to the Nicaraguan Contras on the Front Page of the *New York Times*

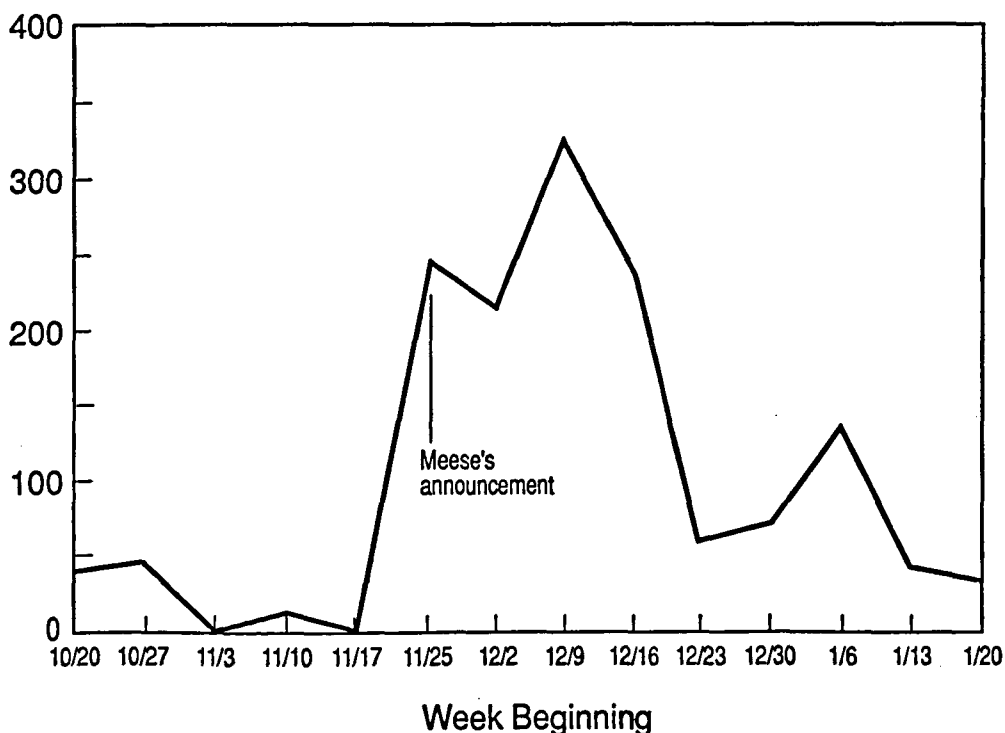
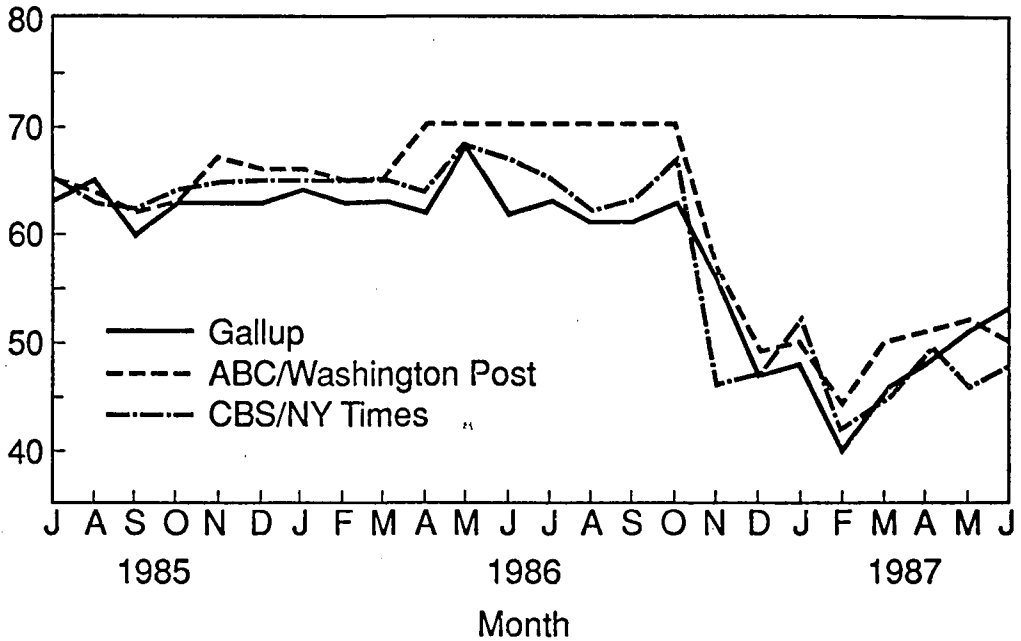


Figure 2. Proportion of the American Public Approving of President Reagan's Job Performance



by the prevailing economic, social, and political conditions of the times. For their knowledge about such conditions, most citizens of course rely on information and analysis provided by mass media. This means that the standards citizens use to judge a president may be substantially determined by which stories media choose to cover and, consequently, which considerations are made accessible. The more attention the news pays to a particular domain—the more frequently it is primed—the more citizens will, according to the theory, incorporate what they know about that domain into their overall judgment of the president. Hence, by calling attention to some matters while ignoring others, news media may alter the foundations of public opinion toward the president. (For a more detailed discussion of priming, see Iyengar and Kinder 1987, chap. 7.)

This central claim of priming has been

supported handsomely in a series of realistic experimental tests (Iyengar and Kinder 1987; Iyengar et al. 1984; Iyengar, Peters, and Kinder 1982). When primed by television news stories focusing on national defense, people judge the president largely by how well he has provided, as they see it, for the nation's defense. When primed by stories about inflation, people evaluate the president by how he has managed, in their view, to keep prices down. The empirical support for priming is strong; but so far it comes entirely from experimental studies. While experiments have genuine advantages, they also suffer inescapable limitations. (See Kinder and Palfrey 1989 for a rigorous defense of experimental methods for political science.) Dependable conclusions about priming—or anything else—are based most securely in corroboration across different methods. So a demonstration of priming in a natural and politically consequential

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setting, free of the limitations of the experimental laboratory, would considerably bolster confidence in the phenomenon.

Priming and the Iran-Contra Connection

The Iran-Contra disclosure provided us with a perfect opportunity to undertake such a test. We do so by exploiting the serendipitous fact that as the attorney general was making his announcement on 25 November, Survey Research Center interviewers were busy questioning citizens all across the country as part of the 1986 National Election Study. That the attorney general's announcement came roughly midway through the 1986 study enables us to see whether, as the theory of priming requires, citizens who happened to be interviewed after the Iran-Contra disclosure evaluated President Reagan more in line with their views on U.S. intervention in Central America than did those (otherwise comparable) citizens who happened to be interviewed before the story broke. If public assessments of the president's performance do indeed depend upon which pieces of political memory come most readily to mind, surely the Iran-Contra disclosure should have enhanced the impact of Central American policy on the public's view of Reagan.

In addition to testing this general hypothesis, we also evaluated three more detailed claims. The first pertains to the degree of correspondence between the news stories that constitute the prime and the opinions that are the target of priming. Several experimental results suggest a specificity to priming—that news coverage influences only the aspects of public opinion that are directly and immediately implicated by the story (see, e.g., Iyengar and Kinder 1987, 106–10). In the Iran-Contra context we would expect priming to be most pronounced on the questions of aid to the Contras and U.S. intervention

in Central America, less evident on the abstract principle of whether the United States should generally intervene in other countries, still less apparent on judgments of U.S. power and prestige in the world, and invisible on matters completely unconnected to the Iran-Contra disclosure, such as the health of the national economy or the desirability of federal programs that assist blacks.

We also used the Iran-Contra revelation to examine whether some citizens are more susceptible to priming than others. We focused in particular on *expertise*. Compared to novices, experts know more about a particular domain; and their knowledge is better organized (Fiske and Kinder 1981). Political experts and novices may react differently to changes in the media's agenda for a number of reasons: (1) because their knowledge is denser and better organized, experts possess a greater and more flexible ability to deal with new information and to interpret it in ways consistent with their prior convictions (Fiske, Kinder, and Larter 1983); (2) because experts possess more informational support for their beliefs, they may be harder to budge; and (3) drawing attention to a particular aspect of national life may only remind experts of what they already know. In a pair of early experiments, experts were indeed relatively immune to priming by television news (Iyengar et al. 1984). But in subsequent experiments (reported in Iyengar and Kinder 1987, chap. 10) this result disappeared, so the significance of expertise in conditioning the impact of news coverage is presently unclear. Here we will see whether novices were more primed by the Iran-Contra revelations than were experts.

Finally, we examined whether news coverage altered the foundations of the public's judgments of President Reagan's *character*, particularly judgments of his competence and integrity in addition to judgments of his performance. The exper-

imental results suggest that the impact of priming on judgments of presidential performance is greater than on judgments of presidential character but that judgments of character also seem to depend to some degree on which aspects of national life news media choose to cover (Iyengar and Kinder 1987, chap. 8). Because the 1986 National Election Study included questions measuring the public's view of President Reagan's competence and integrity, we can pursue these results in the context of the Iran-Contra affair.

Data

Our investigation draws on the 1986 National Election Study (NES) carried out by the Center for Political Studies of the Institute for Social Research at the University of Michigan. Following the November national election, face-to-face interviews were conducted with a national probability sample of 2,176 U.S. citizens of voting age. We confined our analysis to the 1,086 individuals who received Form A of the questionnaire, which included an elaborate assessment of views of President Reagan and a rich battery of questions on foreign affairs in addition to standard questions about the campaign, the candidates, the parties, serious national problems and pressing policy choices, and registration and voting.

In order to test priming, we partitioned the Form A sample into two groups: the first was comprised of the respondents who happened to have been interviewed before the 25 November revelations ($N = 714$); the second was made up of those who happened to have been interviewed afterward ($N = 349$). Respondents interviewed on 25 November were excluded. Initial comparisons revealed that the pre-revelation and postrevelation groups were essentially indistinguishable across a variety of demographic and political comparisons, including education, race, age,

gender, employment status, family income, partisanship, interest in politics, and ideological self-identification. This means that whatever differences between groups we might detect in public opinion toward President Reagan can reasonably be attributed to the Iran-Contra revelations.

We focused on three related but distinct aspects of the public's support for President Reagan: evaluations of his overall performance as president, assessments of his competence, and assessments of his integrity. Table 1 shows that after the Iran-Contra revelations, public support for President Reagan declined across all three. These results are in rough accordance with those reported in various commercial polls bracketing this period (see Figure 2).

Our special interest in these public troubles of President Reagan has to do, of course, with whether such troubles can be connected—in the way specified by the theory of priming—to the Iran-Contra revelations and the avalanche of pictures and stories that they precipitated. Did the revelations enhance the political importance of foreign affairs for the public's assessment of the president? In operational terms, we measured foreign affairs opinions with four questions: (1) whether federal spending on aid to the Contras in Nicaragua should be increased, decreased, or kept about the same; (2) whether the United States should become more or less involved in the internal affairs of Central American countries; (3) whether the United States would be better off not getting involved in the affairs of other nations; and (4) whether the United States' position in the world had grown weaker, stronger, or stayed about the same during the previous year.

Table 1 reveals that public opinion on these matters changed hardly at all in response to the Iran-Contra revelation. Isolationism as a broad stance, attitudes toward U.S. involvement in Central

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**Table 1. Assessments of President Reagan and Opinions on Foreign Affairs
before and after the Iran-Contra Revelation (%)**

Assessment	Prerevelation	Postrevelation
1. Reagan's job performance		
Approve strongly	37.9	32.9
Approve not so strongly	30.0	26.9
Disapprove not so strongly	10.8	13.8
Disapprove strongly	21.2	26.3
2. Reagan's competence		
<i>Intelligent</i>		
Extremely well ^a	22.7	17.5
Quite well	51.2	53.2
Not too well	20.2	20.5
Not well at all	5.9	8.8
<i>Knowledgeable</i>		
Extremely well ^a	29.0	23.4
Quite well	43.8	43.3
Not too well	19.4	21.7
Not well at all	7.7	11.6
3. Reagan's integrity		
<i>Moral</i>		
Extremely well ^a	31.0	28.5
Quite well	52.4	46.5
Not too well	12.8	18.3
Not well at all	3.8	6.6
<i>Decent</i>		
Extremely well ^a	38.5	33.9
Quite well	51.0	49.6
Not too well	8.2	11.2
Not well at all	2.3	5.3
4. Aid to Contras in Nicaragua		
Increase support	9.1	7.0
Same	23.6	21.6
Decrease support	67.3	71.4
5. U.S. involvement in Central America		
Much more involved	5.2 } 5.5 } 10.1 }	6.4 } 7.1 } 9.4 }
	20.8	22.9
In between	23.4 } 17.2 } 17.2 }	25.6 } 17.8 } 16.5 }
	55.7	51.5
Much less involved	21.3 }	17.2 }
6. Isolationism		
Agree (U.S. should stay home)	30.8	29.2
Disagree (U.S. should not stay home)	69.2	70.8
7. U.S. position in the world		
Stronger	21.9	14.6
Same	40.5	40.3
Weaker	37.6	45.1
Number of cases	714	349

Source: 1986 National Election Study.

^aThe question asked how well the words *intelligent*, *knowledgeable*, *moral*, and *decent* described Reagan.

America in general, and willingness to support more assistance to the Contras were all apparently unaffected by news of the diversion of funds ($p > .35$ in all cases), although more citizens than before claimed afterward that the United States' position in the world was weakening (45.1% vs. 37.6%, $p < .02$). This one shift probably has less to do with the Iran-Contra disclosure than with the widespread perception in the public that the Reagan administration had undertaken an arms-for-hostages deal with Iran. In any case, priming may equally occur in the absence or presence of change in the foundational elements of the public's presidential evaluations.

Results

To examine priming empirically, we must first specify a model of public support for the president relevant to the case at hand. In formal terms, the model is given by the following equation:

$$\begin{aligned} \text{Overall Performance} = & b_0 + b_1 \\ & (\text{Contras and Central America}) \\ & + b_2 (\text{isolationism}) + b_3 (\text{U.S.} \\ & \text{strength}) + b_4 (\text{national economic} \\ & \text{assessments}) + b_5 (\text{aid to blacks}) \\ & + b_6 - b_{15} (\text{control variables}). \end{aligned} \quad (1)$$

Notice that equation 1 includes *three* variables to represent the domain of foreign affairs, not four. In preliminary analyses, we found that attitudes toward U.S. involvement in Central America and attitudes toward aid to the Contras were highly correlated. People who opposed aid to the Contras were likely to oppose U.S. involvement in Central America, and those who favored aid to the Contras were likely to favor U.S. involvement in Central America ($r = .42$). These two attitudes correlated more weakly with views on isolationism and on U.S.

strength ($.10 < r < .25$), and these latter two attitudes were uncorrelated with each other ($r = .04$). Therefore, in the analysis of priming, we averaged attitudes toward aid to the Contras and toward U.S. intervention in Central America into a single measure.

In addition to the three measures of opinion on foreign affairs, equation 1 also includes a measure of the citizen's assessment of national economic conditions (an average of the citizen's perception of change over the past year in unemployment, inflation, and the general economy) and a measure of the citizen's opinion regarding the desirability of federal programs that provide assistance to blacks (averaged across two questions). We included national economic assessments and race policy views because both are highly relevant to presidential evaluations (e.g., Fiorina 1981; Kinder, Adams, and Gronke 1989; Rosenstone 1983) and because they are utterly unrelated to the Iran-Contra revelation. We expected that the impact of national economic assessments and race policy views on evaluations of President Reagan should either be unaffected by the revelation or should decline, a reflection of the media's sudden preoccupation with Central America. Finally, equation 1 also includes a standard set of background variables important for control purposes though of little substantive interest in their own right: employment status, age, race, gender, region, education, income, and party identification.³

To test the basic claim of priming, we estimated equation 1 twice, first based on respondents in the prerevelation group, then based on respondents in the post-revelation group. Priming insists first of all that the impact on assessments of President Reagan's performance due to foreign affairs opinions—indexed in equation 1 by b_1 , b_2 , and b_3 —be greater in the postrevelation group than in the prerevelation group. Priming also requires that

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Table 2. Estimated Impact of Public Opinion on Assessments of President Reagan's Performance, before and after the Iran-Contra Revelation (Unstandardized OLS Regression Coefficients)

Opinion Domain	Prerevelation	Postrevelation	Difference	Significance of Difference ^a
Contras-Central America	.18*	.29*	.11	.17
Isolationism	.02	.10*	.08	.02
U.S. strength	.14*	.15*	.01	.45
Economic assessments	.33*	.35*	.02	.36
Aid to blacks	.22*	.00	-.22	.05
Number of cases	607	296	—	—

Source: 1986 National Election Study.

^aEntries in this column are one-tailed *ps*.

**p* < .05 (one-tailed).

the impact of economic assessments and racial attitudes on evaluations of Reagan—indexed by b_4 and b_5 —should remain the same or decline across the two groups.

The results of estimating equation 1 with Ordinary Least Squares (OLS) regression are presented in Table 2. To interpret the coefficients shown there, keep in mind that all variables were coded to range from 0 to 1, with 1 indicating (1) favorable evaluations of Reagan's performance, (2) support for the Contras and for U.S. involvement in Central America, (3) support for interventionist foreign policy in general, (4) the view that the U.S. had grown stronger in the world, (5) belief that national economic conditions had improved over the previous year, and (6) opposition to federal aid to blacks, respectively. As Table 2 reveals, public opinion on foreign affairs did indeed become more important for assessments of the president's performance in the immediate aftermath of the Iran-Contra disclosures. Foreign affairs attitudes were relatively unimportant to the public's view of Reagan's performance prior to 25 November (see Table 2, col. 1). After 25 November, however, the story is very different—foreign affairs loomed rather large

in the public's presidential assessment (see Table 2, col. 2).

Moreover, the sharpest increases in Table 2 appear for the aspects of public opinion on foreign affairs most immediately implicated by the revelations. The importance of public opinion on the question of assistance to the Contras and U.S. intervention in Central America increased substantially from the prerevelation period to the postrevelation period (the unstandardized regression coefficient went from .18 to .29), as did the importance of the public's view of the general choice between intervention and isolationism (from .02 to .10). Meanwhile, the public's view of the strength of the United States around the world was evidently unaffected by the revelation (.14 vs. .15). This pattern of results corroborates the experimental findings noted earlier. Both suggest that priming requires a close correspondence between the news stories that do the priming and the opinions that are primed.⁴

The theory of priming predicts not only that public opinion on foreign affairs will become more important for presidential assessments after the Iran-Contra disclosure, but also that aspects of public opinion relevant to the president's success

but unrelated to the disclosure will not become more important. The results in Table 2 confirm this prediction as well. First, citizens' assessments of national economic conditions contributed sizably to their view of President Reagan's performance—but did so equally before and after 25 November (.33 vs. .35). Second, citizens' views on race policy were evidently shunted aside (if momentarily) by the media's sudden preoccupation with Central America. Prior to 25 November, citizens who opposed federal programs designed to help blacks were more likely than citizens who supported them to support President Reagan. After 25 November, however, this political difference over race no longer contributed to public differences over the president's performance (.22 vs. .00). It would appear that the disclosure of 25 November altered the foundation of support for the president both by bringing certain aspects of public life to center stage and by pushing other aspects of public life off the stage altogether.

In order to illustrate the magnitude of the priming effects documented in Table 2, we generated predicted evaluations of Reagan, first using the prerevelation group's regression coefficients and then using the coefficients estimated with the postrevelation group. This required us to specify values of all the predictor variables in equation 1. For this purpose we chose to represent an average, middle-of-the-road U.S. citizen: a white, female, forty-year-old high school graduate from the Midwest with an annual family income of 22 thousand dollars, a political independent, who believed that national economic conditions had changed little over the previous year and who neither favored nor opposed federal programs for blacks. We carried out this exercise twice: once assuming that our hypothetical average citizen held views on foreign affairs that would predispose her to support Reagan (favored aid to the Contras and U.S. involvement in Central America,

favored international interventionism generally, and believed the United States was maintaining its international strength) and once assuming she held views on these issues that would move her in the opposite direction (opposed aid to the Contras and U.S. intervention in Central America, favored isolationism, and believed that the U.S. had lost international strength).

The results of this simulation suggest that the consequences of priming for presidential support are contingent on the citizen's prior views. Among (typical) citizens predisposed to support Reagan on foreign policy grounds, the effects of priming were negligible. Equation 1 predicts evaluation of President Reagan's performance by such people to be .73 (on the zero-to-one scale) prior to the Iran-Contra revelation and .75 afterward. But among typical citizens predisposed to *oppose* Reagan on foreign policy grounds, the story is very different: equation 1 now predicts a Reagan evaluation of .53 before the revelation and only .38 after, a steep falloff in support. Thus, the effect of priming was to reduce assessments of President Reagan's performance among critics of U.S. policy in Central America substantially.

In the 1986 NES data, the prerevelation group's average evaluation of Reagan (on the zero-to-one scale) was .602 whereas the postrevelation group's was .548. How much of this .054 decrease can be attributed to priming? We used each of the 1986 NES respondents' actual demographics and attitudes to generate two predicted Reagan evaluations: one using the prerevelation regression weights, the other using the postrevelation regression weights. The average predicted prerevelation evaluation was .532 and the average predicted postrevelation evaluation was .499, a difference of .033. Thus, almost two-thirds of the decrease in this sample's Reagan approval ratings can be attributed to priming.

Our next move was to investigate

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Table 3. Estimated Impact of Public Opinion on Assessments of President Reagan's Performance before and after the Iran-Contra Revelation, Separately for Political "Novices" and Political "Experts" (Unstandardized OLS Regression Coefficients)

Opinion Domain	Prerevelation	Postrevelation	Difference	Significance of Difference ^a
Among Political "Novices"				
Contras-Central America	.12*	.35*	.23	.06
Isolationism	.01	.08*	.07	.09
U.S. strength	.14*	.18*	.04	.32
Economic assessments	.29*	.39*	.10	.19
Aid to blacks	.20*	.03	-.17	.05
Number of cases	383	191	—	—
Among Political "Experts"				
Contras-Central America	.22*	.28*	.06	.43
Isolationism	.06	.20*	.14	.05
U.S. strength	.16*	.06	-.10	.29
Economic assessments	.39*	.41*	.02	.41
Aid to blacks	.20*	.07	-.13	.37
Number of cases	222	105	—	—

Source: 1986 National Election Study.

^aEntries in this column are one-tailed *ps*.

**p* < .05 (one-tailed).

whether political novices were more susceptible to priming than political experts. Following Zaller's (n.d.) advice, we assessed expertise using measures of objective knowledge about political affairs. In particular, we partitioned the 1986 NES respondents according to their success at identifying six political figures: George Bush, Caspar Weinberger, William Rehnquist, Paul Volker, Robert Dole, and Tip O'Neill. The 65% who correctly identified three or fewer were considered novices, and the 35% who correctly identified four or more were considered experts. Then we simply repeated the analysis summarized in Table 2, separately within each group.⁵

Among political novices, the Iran-Contra revelation had a substantial priming effect (see the upper panel of Table 3). Opinions on foreign affairs were more important in novices' assessments of Reagan's performance after the revelation

than before, whereas national economic assessments were no more important, and opinions on federal programs to aid blacks were less important. Particularly noteworthy was the dramatically enhanced prominence of Central American policy in novices' presidential evaluations after 25 November (the regression coefficient nearly tripled, from .12 to .35). As a general matter, the pattern of priming effects noted for the public as a whole is maintained and sharpened among novices alone.

Meanwhile, priming was less apparent in the assessments of President Reagan's performance offered by political experts (see the lower panel of Table 3). Indeed, priming among experts seems confined to a single aspect of opinion. In the wake of the Iran-Contra disclosure, experts were more likely to evaluate President Reagan according to their general views on U.S.

**Table 4. Estimated Impact of Public Opinion on Assessments of
President Reagan's Character before and after the Iran-Contra Revelation
(Unstandardized OLS Regression Coefficients)**

Opinion Domain	Prerevelation	Postrevelation	Difference	Significance of Difference ^a
Assessing Reagan's Competence				
Contras-Central America	.09**	.20**	.11	.15
Isolationism	.04**	.09**	.05	.04
U.S. strength	.12**	.06*	-.06	.06
Economic assessments	.10**	.07	-.03	.07
Aid to blacks	.14**	.12**	-.02	.41
Number of cases	632	304	—	—
Assessing Reagan's Integrity				
Contras-Central America	.03	.12**	.09	.14
Isolationism	.05**	.06**	.01	.21
U.S. strength	.07**	.07**	.00	.47
Economic assessments	.13**	.13**	.00	.39
Aid to blacks	.05*	.04	-.01	.28
Number of cases	629	303	—	—

Source: 1986 National Election Study.

^aEntries in this column are one-tailed *ps*.

**p* < .10 (one-tailed).

***p* < .05 (one-tailed).

intervention in international affairs (.06 vs. .20). It is interesting both that experts seem generally less susceptible to priming and that the character of priming among experts is different. Novices appear to be primed on those aspects most directly and concretely implicated by the news coverage while experts, insofar as they are primed at all, are influenced at a more abstract level.⁶

The distinction between novices and experts apparent in Table 3 is consistent with our experimental results (reported in Iyengar et al. 1984) and with other aspects of the NES survey data as well. In particular, the decline in support for President Reagan's performance in the immediate aftermath of the attorney general's disclosures registered in the public as a whole was greater among novices than among experts. Of the novices, 64.8% approved of Reagan's performance prior to the Iran-

Contra revelations while just 57.4% did so afterward, a net change of 7.4 percentage points. Meanwhile, 67.6% of the experts approved of Reagan's performance before the revelations, and 65% did so afterward, a net change of 2.6 percentage points. Novices were more likely than experts to be swept away by the avalanche of stories and pictures set in motion by the 25 November revelations.

Finally, we examined whether media coverage of the Iran-Contra connection altered the foundations of the public's assessments of President Reagan's character. To do so, we reestimated equation 1, first with assessments of Reagan's competence, then with assessments of his integrity, as the dependent variable.⁷ The results shown in Table 4 replicate, in a somewhat attenuated fashion, those reported earlier regarding the public's assessments of President Reagan's per-

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formance. For judgments of character, as for judgments of performance, opinions on Central American policy became more important after the Iran-Contra revelation than before while assessments of national economic conditions and views on government race policy became, if anything, less important. The effects of priming were a bit more pronounced in the public's judgments of Reagan's competence than in judgments of his integrity. Indeed, apart from the increased importance of views on Central American policy, the public's assessment of Reagan's integrity appears quite unmoved by the Iran-Contra revelation. The greater susceptibility of competence assessments apparent in Table 4 may reflect, in part, how the news media began to frame the Iran-Contra story, namely, as exposing Reagan's disengagement from U.S. foreign policy. Framed in this way, the Iran-Contra story naturally implicated the president's competence more than his integrity.

Discussion

The disclosure in the fall of 1986 that funds received from the sale of arms to Iran had been secretly channeled to the Nicaraguan Contras provides an excellent opportunity to test the theory of priming in a politically consequential setting. By and large, the theory stands up well to this examination. Citizens questioned after the revelations held President Reagan to an altered set of standards, and these alterations can be directly traced to the changes in the media's agenda provoked by the Iran-Contra revelations. As expected, priming was particularly pronounced for the aspects of public opinion most directly implicated by the news coverage (aid to the Contras and involvement in Central America), was more apparent in the judgments of political novices than in the judgments of political experts, and showed up more clearly in

the public's evaluations of President Reagan's overall performance than in assessments of his character. Taken together, these results strongly support the theory of priming and are important not least because they escape the artificiality that inevitably accompanies experimental laboratory research, which had provided priming's sole empirical support up until now. Our confidence in priming is fortified by the close convergence between previous experimental results on the one hand and the results reported here, based on personal interviews with a national sample of citizens responding to a real crisis, on the other.

The comparative advantages of the present investigation—representative sampling, professional interviewers, careful and elaborate pretesting, the serendipitous intrusion of a dramatic and heavily covered event—are real enough; but we should also acknowledge some comparative disadvantages as well. As we noted earlier, the prerevelation group closely resembles the postrevelation group in terms of basic demographic and political characteristics. However, the two groups could still differ from one another in consequential ways that we missed. Given the present design, we cannot be certain that the differences we observed between the prerevelation and postrevelation groups in their assessments of President Reagan were actually due to priming and not to some preexisting and unmeasured difference or to some event other than the Iran-Contra revelation. Notice that this worry is swept aside by the procedure of random assignment that is the heart of the experimental method. It is the convergence of results across different methods of testing that is crucial in science in general and crucial to the standing of the theory of priming in particular.

Priming provides an empirically grounded, psychologically plausible account of how individuals form and revise their views of presidential performance.

Priming therefore aspires to complement the dominant tradition in research on support for the president, which applies time series methods to estimate the impact of national and international events on change in the public support. This research has been enormously informative about the aggregate effects of such events while revealing little about the diversity among citizens' reactions that seem certain to underlie change and stability in the aggregate. From such research we know a great deal about how the public as a whole will respond to a change in unemployment or to a dramatic international crisis but virtually nothing about which citizens are most likely to increase their support for the president, which are likely to decrease their support, and which are likely to be unmoved. The literature is even less prepared to tell us *why* different citizens respond differently to the same event. Priming provides a general framework to answer such questions.

Priming also carries an implication for the study of political change more generally. Most quantitative studies of this sort assume that the effects of economic and social change are constant over time—that, say, the impact of a one-percentage-point change in the unemployment rate on the incumbent's reelection chances is invariant across history (or, less dramatically, the length of the time series). Priming collides head-on with this assumption. According to priming, shifts in news media content alter the political importance that the public attaches to the flow of events. If priming is pervasive, the assumption of constant effects seems dubious. This does not mean that the typical time series analysis of political change is worthless, only that such analysis would be enhanced by incorporating the fundamental insight of priming—that through its monopoly over the immediate telling of political history, media possess the power to influence what the

public considers and what it ignores.

A final implication of our findings involves stepping back and taking a normative stance on the matter of political change. Exposure to political information through mass media varies enormously across the U.S. public. Some citizens are constantly preoccupied with the flow of political news while others are utterly indifferent to it. Not surprisingly, those who are most heavily exposed to political news also accumulate the most political knowledge. The devoted viewer, listener, and reader becomes society's political expert. With this in mind, we might expect that it would be the expert who would be most influenced by changes in the media's agenda. In the case of the Iran-Contra affair, the news media's daily updating would presumably have the greatest impact on citizens who absorbed the complete story and the least impact on those exposed only to fragments of it. Our results suggest just the opposite. Citizens with the least knowledge (and presumably the least exposure) manifested the largest priming effects. Thus, change in support for the president in response to the Iran-Contra revelations appears to have been dominated by the least-informed, a result that has troubling implications for the exercise of power in Washington. Presidents who enjoy popular support typically have success in shaping the political agenda of the nation (Kernell 1986; Rivers and Rose 1985). Our findings suggest that change over time in popular approval—and thus the waxing and waning of presidential power—may depend the most on the citizens who know the least.

Notes

An earlier version of this paper was presented at the 1988 annual meetings of the American Political Science Association, Washington. It is based in part on research supported by a grant to Kinder from the National Science Foundation (SES-8511029). We are grateful to Nancy Brennan and Judith Ottmar for

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their help in preparing the manuscript; Janet Attarian and Konnie Toth for their careful coding of the *New York Times*; and Shanto Iyengar, Steven Rosenstone, and Janet Weiss for their good advice. The public opinion data reported here were originally collected by the Center for Political Studies of the University of Michigan, Institute for Social Research, on behalf of National Election Studies. Krosnick and Kinder are equally responsible for the analyses and interpretations presented here.

1. Most, but not quite all, the time series work presumes that citizens respond uniformly to whatever is happening in the country. The conspicuous and excellent exception is provided by Hibbs, Rivers, and Vasilatos (1982a), who found that citizens' reactions to events were conditioned by their class affiliations and partisan attachments.

2. Figure 1 displays the number of front page column-lines (text, headlines, and pictures) mentioning the Nicaraguan rebels, Nicaragua, or the Contras.

3. Employment status was coded 0 for respondents who were looking for work and 1 for respondents who were not. Age and educational attainment were coded in years. Income was coded 1 to 22, representing 22 separate income categories. Race was coded 1 for whites and 2 for nonwhites. Gender was coded 1 for males and 2 for females. Party identification was represented by two dummy variables: the first was coded 1 for Democrats and 0 for all other respondents; the second was coded 1 for Republicans and 0 for all other respondents. Region was represented by three dummy variables: the first was coded 1 for residents of central states and 0 for all others; the second was coded 1 for residents of southern states and 0 for all others; and the third was coded 1 for residents of western states and 0 for all others.

4. To test the statistical significance of the changes in the coefficients across the pre- and postrevelation groups, we estimated an enhanced version of equation 1. In particular, we added the following variables to it: (pre-post), (pre-post)(Contras-Central America), (pre-post)(isolationism), (pre-post)(U.S. strength), (pre-post)(national economic assessments), (pre-post)(aid to blacks). Pre-post is a dummy variable coded zero for the prerevelation group and one for the postrevelation group. Thus, the coefficients associated with each of the multiplicative terms tests whether the impact of each aspect of public opinion on evaluation of Reagan's performance differs from the prerevelation group to the postrevelation group. The significance levels of these coefficients appear in the text and in the far right-hand column of Table 2.

5. We set the dividing line between experts and novices in this fashion because it generated as close to equal-sized groups as possible while making the experts more rare than the novices, a distribution that suits current wisdom about the distribution of

political expertise (see Kinder and Sears 1985). Operationalizing expertise in terms of general knowledge about politics differs from the way we have operationalized it in our previous studies of priming. There, we used measures of domain-specific knowledge (e.g., Iyengar and Kinder 1987; Iyengar et al. 1984). In the present case, we would have preferred to use measures of knowledge about Central American affairs or about Nicaragua in particular, had such measures been included in the 1986 NES.

6. To assess the statistical significance of the differences associated with expertise, we estimated an enhanced version of the equation described in n. 4. To that equation we added six new terms: (expertise), (expertise)(pre-post)(Contras/Central America), (expertise)(pre-post)(isolationism), (expertise)(pre-post)(U.S. strength), (expertise)(pre-post)(national economic assessments), (expertise)(pre-post)(aid to blacks). Here we treated political expertise as a continuous variable defined as the proportion of the six political figures correctly identified. The coefficients associated with the multiplicative terms then assess whether the magnitude of the pre- to postrevelation difference in each attitude's impact depends upon expertise. When we estimated this enhanced equation, we found that the three-way interaction involving attitudes toward Central American policy and that involving views on isolationism were both marginally significant ($p = .09$ and $.14$ respectively) but that the remaining three three-way interactions were not ($p > .25$ in each case).

7. The measure of competence is an average of respondents' judgments of how well the terms *intelligent* and *knowledgeable* describe Reagan; the measure of integrity is an average of comparable judgments regarding *moral* and *decent*.

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PRESIDENTIAL COATTAILS IN SENATE ELECTIONS

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Despite the diminished importance of partisanship, greater split-ticket voting, and a growth in Senate campaign spending, a party's presidential vote in the states remains positively related to its Senate vote in recent elections. We investigate to what extent presidential coattails are responsible for this association. State election returns for Senate and presidential contests are examined in presidential election years from 1972 to 1988. The analysis indicates that (1) presidential coattails exert a modest but significant influence on the Senate vote, probably affecting the election outcomes in twelve cases, and (2) partisanship remains a significant linkage between presidential and Senate elections.

Only presidential elections are better financed, are more competitive, involve more experienced and well-known candidates, and receive more media and public attention than U.S. Senate elections. As Abramowitz (1988, 385) aptly summarized, "The relatively small number of senators, the size and political importance of Senate constituencies, the length of Senate terms, and the special constitutional responsibilities of the Senate all contribute to the political visibility of individual senators." Much the same can be said for the visibility of Senate candidates generally.

The salience of Senate elections is evidenced in a comparison with House elections. Senate candidates are generally more familiar to the public than House candidates (Hinckley 1980, 446; Hinckley 1981, 23; Jacobson 1987, 111; Stokes and Miller 1982). They also spend much more on their campaigns. In 1986 the typical Senate candidate spent nearly 10 times what the typical House candidate spent. Senate seats are also generally more com-

petitive than House elections. Fewer Senate incumbents go unchallenged; the rate of incumbent reelection is lower in the Senate than in the House (75% vs. 94% from 1968 to 1986); and the reelection vote margin of incumbents in the Senate is lower than in the House. From 1968 to 1986, nearly three-quarters of House incumbents were reelected with 60% or more of the vote while less than half of reelected senators won by that margin (Ornstein, Mann, and Malbin 1987, 59-60).

The salience of Senate elections is also indicated by their ability to draw voters to the polls. By Caldeira, Patterson, and Markko's (1985, 505) estimates, a concurrent Senate election typically boosts turnout by about six percentage points. Although this is roughly half the turnout boost provided by a presidential race, it is, nevertheless, substantial and reflects the considerable importance attached to Senate elections by the electorate.

The salience attached to Senate elections raises an interesting question about

potential presidential coattail effects. Are Senate campaigns, like contests for the House and offices of lesser salience, subject to the influence of presidential coattails? Or are they free of coattail effects because of their greater public salience, financing, and visibility and the prominence of the candidates?

While significant presidential coattails have been found in House and state legislative elections (J. Campbell 1986a, 1986b; Born 1984; Calvert and Ferejohn 1983), research on presidential coattails in Senate elections has been relatively meager. Key (1958, 592-99) found that parties were able to hold or win a greater share of seats in states in which the presidential candidate ran well. Hinckley (1970), examining Senate contests in 34 competitive states from 1956 to 1966, found that deviations of the presidential and Senate votes from the states' simulated base party vote were positively correlated ($r = .70$). Deviations from the base vote were examined as a rough control for state partisanship. More recently, Stewart (1987) examined a general model of Senate voting, considering the vote-drawing powers or turnout effects of incumbents and challengers separately. He found that presidential coattails significantly affected the numbers of voters turning out to vote for both incumbents and challengers. The most comprehensive study of Senate elections to date (Abramowitz 1988), however, did not explicitly specify any presidential coattail effects.¹

Of course, it is possible that presidential coattails do not extend to Senate candidates. While evidence of presidential coattail effects have been found below the level of Senate elections, these coattail effects may have diminished somewhat in recent years (Campbell 1986b; Ferejohn and Calvert 1984). Moreover, Senate races may have become so visible and candidate-centered that the association of a Senate candidate with a presidential candidate now conveys little new infor-

mation to voters. Partisan associations linking candidates to each other as well as framing the public's view of campaigns may also have weakened sufficiently to further trim coattails.

Whatever relationship might exist between the presidential and Senate votes may also be the result of common causes rather than presidential coattails. Both votes ought to be affected by long-term partisan and ideological divisions. A party's presidential and Senate candidates ought to do better in states hospitable to the party and more poorly in states hostile to it.

This research attempts to determine to what extent and how presidential and Senate voting are related. More specifically, to what extent, if any, do presidential candidates offer coattails to Senate candidates?

Data and Variables

The data are from individual Senate elections held in presidential election years from 1972 to 1988. The starting point of 1972 is dictated by the lack of campaign finance data for earlier Senate elections. A total of 164 separate Senate elections were held in these five presidential election years. After excluding six seats uncontested in either the current or prior election, six races for which no comparable state partisanship data are available (Hawaii and Alaska), and two cases in which the state had cast a substantial presidential vote in a prior election for a third-party candidate, a total of 150 elections are left in the analysis. Of these 150 contests, Democratic incumbents defended their seats in 62 cases, Republican incumbents ran in 56 cases, and the remaining 32 cases were open seats.

Abramowitz (1988), Stewart (1987), and others have identified a number of influences on the Senate vote. These will be taken into account in examining for presi-

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dential coattail effects. These variables are of two types: those suspected of influencing both the presidential and Senate votes and those suspected of influencing only the Senate vote.² Three variables are specified as common causes of the presidential and Senate votes: the state's partisanship, its ideology, and the general trend of greater support for Republican candidates throughout this period.³ Four variables are presumed to influence only the Senate vote: incumbency, the prior vote for the Senate seat, the relative campaign spending of the candidates, and the divisiveness of the Senate primary elections.

Presumably, more Democratic and more liberal state electorates should cast a greater share of votes for both Democratic and presidential and senatorial candidates. On the Senate side, Abramowitz (1988) found significant partisanship and ideological effects. On the presidential side, the impact of partisanship on the presidential vote is writ in stone (A. Campbell et. al. 1960; Converse 1966; Rosenstone 1983, 46) and Rabinowitz, Gurian, and McDonald (1984) found the state-level presidential vote from 1944 to 1980 to be structured by both partisanship and ideology. The measures of both partisanship and ideology are taken from Wright, Erikson, and McIver's (1985) analysis of CBS-*New York Times* poll data in the states (their unweighted measure of the active electorate).⁴ The state Democratic partisanship measure is constructed from their state distributions of partisans. It is the percentage of Democratic identifiers in the state plus one-half of the independents and nonrespondents. The ideological tendencies of the states are measured as the percentage of a state's sampled population that claimed a liberal political orientation. Since both state partisanship and ideology are measured as constants for each state across this period, their effects on the Senate vote are specified as indirect

—through the presidential vote, the prior Senate vote, and campaign finance advantages—as well as direct.

The presidential and Senate votes may also be related by a common trend. Bullock (1988), Petrocik (1987), Stanley (1988) and others have concluded that a secular realignment, most evident in southern states, has been underway throughout this period. This may be partly reflected in the string of Republican presidential victories since 1968, broken only by Carter's narrow win in 1976, following Watergate. It is also evident in congressional elections, despite the fact that the Democrats have controlled the House throughout this entire period and the Senate for most of it. The nationwide Democratic vote for the House exhibits a noticeable downward tendency throughout this period. The correlation between the election year and the national Democratic House vote for the eight congressional elections from 1972 to 1986 was quite strongly negative ($r = -.46$ and $-.72$ from 1974 to 1986). Of course, the trend favoring the Republican party may be accounted for in different terms. The Republicans, as the minority party, should be helped by a dealigning of the party system as well as by its realignment. The majority party has more to lose in the general weakening or breaking of party ties in a dealignment. For present purposes, it is enough to note that whether by realignment, dealignment, or both, the Republican party has enjoyed an electoral trend in its favor. However caused, a significant trend affecting both presidential and Senate votes may be responsible for some part of the correlation between the two votes and therefore must be taken into account. The trend variable in this analysis is simply a two-digit coding of the election year (e.g., 1972 = 72).

Abramowitz (1988), Kostroski (1973), and others have found that the wide variety of advantages and resources available to Senate incumbents has a con-

siderable impact on Senate elections. Based on these prior estimates of incumbency advantage, the Democratic vote should be greater for seats defended by Democratic incumbents and lower for seats defended by Republican incumbents. The incumbency variable is +1 when a Democrat incumbent is seeking reelection, -1 when a Republican incumbent is seeking reelection, and zero in open seat contests.⁵

A Senate vote may also be influenced by the vote in the previous election for that seat. Democrats may be expected to do well where they did well in the past and poorly where they did poorly in the past. In all but the few cases requiring special elections, the prior Senate vote was the statewide Democratic Senate vote six years before the current election year.

There is little question that the relative campaign spending of Senate candidates affects the Senate vote. The measure of campaign spending advantage used here is the difference between Democratic and Republican spending as a proportion of the total campaign spending by both major party candidates. Values range from +1 when all spending is by the Democrat to -1 when all spending is by the Republican.⁶

This measure has several virtues.

1. It directly compares one candidate's spending to his opponent's. The absolute value of a dollar spent in behalf of a candidate is presumed to be the same as a dollar spent in opposition to that candidate.
2. It controls for the expense of campaigns in various states and under varying circumstances; that is, it is undoubtedly more expensive to run a typical Senate campaign in New York than in Wyoming. The question is, How much more expensive is it to run a comparable campaign? The total spending of the two candidates, the index's denominator, should take this difference into account.
3. Since the numerator and the denominator are in terms of the same dollars, there is no need to adjust for inflation. The index has the same value whether nominal or constant dollars are used.
4. It incorporates the notion of diminishing

returns (Jacobson 1980, 40), without resorting to various curvilinear functions (e.g., natural logarithms, squared terms) that are less readily interpretable. As total spending increases, the impact of each additional dollar in either candidate's campaign should make less of a difference and this is reflected in the campaign spending advantage index.

The comparative campaign spending advantage is specified as having both an additive and an interactive effect. As Abramowitz (1988, 393), Jacobson (1978, 41), and others have shown, campaign finances are more important to challengers than to incumbents, since challengers must compensate for many advantages (e.g., voter recognition) that incumbents enjoy by virtue of having previously sought, won, and served in office. Given the coding of Democratic incumbents (1), the interaction of incumbency status and the campaign spending advantage index is expected to have a negative effect on the Democratic Senate vote.

If candidates face serious opposition in obtaining their party's nomination, they may be weakened in their general election bid. Divisive primary battles may deplete resources that would have otherwise been available for the general election campaign; create negative impressions about the eventual nominee (perhaps given greater credence since the attacks come from within the nominee's own party); and leave many of the party identifiers of the eventual nominee's party disappointed that their first choice failed to win the nomination (Southwell 1986). Previous research on divisive primaries has been inconclusive. Several studies conclude that divisiveness does no harm to a candidate's general election performance (Hacker 1965; Kenney 1988; Miller, Jewell, and Sigelman 1988; Piereson and Smith 1975). Others, however, find evidence that divisive primaries, as expected, weaken the candidate in the general election (Bernstein 1977; Born 1981; Kenney and Rice 1987; Lengle 1980). Given the inconclusive nature of

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this research, it is appropriate to entertain the possibility of divisive primary effects.

Of course, all primary challengers are not equally damaging to, or indicative of, a candidate's general election prospects. Some challenges present only token opposition, indicate no real weakness of the eventual nominee, and arguably may strengthen the nominee's image as a viable candidate. Thus, the mere fact of a primary challenge does not measure the severity of the challenge. The severity of the challenge is measured as the winning candidate's margin over the closest rival. Following the logic of comparative advantage used in devising the campaign spending index and the previous work of Kenney and Rice (1987, 35), an index of the primary divisiveness advantage is computed as the difference between the Democratic nominee's primary margin and the Republican nominee's primary margin. Unchallenged nominees are scored as having a margin of 100 percentage points. The divisiveness advantage measure ranges from 100 (when the Democrat is unchallenged for the nomination and the Republican very narrowly wins nomination) to -100 when the Democrat squeaks by and the Republican goes unchallenged. If primary divisiveness is harmful to candidates, it should be reflected in significant positive effects of the variable as constructed.

Findings

The Presidential and Senate Votes

Are the presidential and Senate votes positively associated? Is there a relationship to explain? The two votes are, in fact, positively correlated over this period ($r = .37$; $b = .59$ with the Senate vote as the dependent variable).⁷ The average Democratic Senate vote is about 53% in states carried by the Democratic presidential candidate. The typical Democratic

Senate vote is a good bit lower in states where the party lost badly in the presidential race. Where Democrats received 40% or less of the presidential vote, the average Democratic Senate vote was less than 45%. Although the presidential and Senate votes are far from identical, they are nevertheless related. The question of how they are related can now be addressed.

Coattails

The regression results are presented in Table 1. These findings indicate that *presidential coattails do affect the vote for Senate*.⁸ While not strong, presidential coattails are also not inconsequential. The presidential vote coefficient, as expected, is positive and statistically significant. A 10-percentage-point gain in a party's presidential vote in a state, according to this estimate, adds about two percentage points to the vote for its Senate candidate. Put differently, about 18% of the presidential vote carries over to the Senate vote.

The Ordinary Least Squares (OLS) regression estimate of Table 1 assumes that the Senate vote does affect the presidential vote, that there are no "reverse" coattails. However, given the salience of Senate elections, this may not be an entirely safe assumption. Allowing for the possibility of Senate coattails in presidential contests, an overidentified Two Stage Least Squares (2SLS) nonrecursive analysis was also conducted (J. Campbell and Summers 1989). The 2SLS results confirm the OLS findings. The 2SLS estimates of presidential coattails ($b = .16$, $t = 1.62$) are nearly equal to the OLS estimates and yield no evidence of Senate coattails in presidential races ($b = .02$, $t = .46$).

How much of a difference do presidential coattails make in Senate elections? The answer to this question depends in part on the magnitude of the presidential vote coefficient, but it also depends on the

**Table 1. OLS Regression Coefficient Estimates for the
Democratic Senate Vote**

Variable	Coefficient	Standardized Beta	t-score
Constant	45.13	—	3.99*
Presidential vote	.18	.11	1.89*
Prior Senate vote	.23	.21	3.05*
Senate incumbency advantage	1.09	.08	.99
Senate spending advantage	13.85	.57	8.21*
Spending interaction with incumbency	-1.34	-.04	.82
Senate primary divisiveness	.01	.06	.96
Trend (year counter)	-.19	-.09	1.74*
State partisanship	-.04	-.02	.33
State liberalism	.16	.05	.86
Number of cases		150	
R ²		.69	
Adjusted R ²		.67	
Standard error		7.13	

Note: The dependent variable is the Democratic percentage of the U.S. Senate vote by state. All variables except the trend and interaction variables are constructed so that more positive scores are favorable to Democrats.

* $p \leq .05$, one-tailed test.

variability of the presidential vote and the baseline from which the variation is measured. The natural baseline for the two-party presidential vote, given the inclusion in the model of controls for the normal partisan state vote, is the 50% mark. Presidential candidates who win more than 50% of a state's two-party presidential vote can help their party's Senate candidate. In the elections examined here, the mean absolute presidential vote deviation from an even split of the vote was 8.6 percentage points. Given the estimate of coattail effects ($b = .18$), this translates into a mean coattail effect of 1.5 percentage points of the Senate vote. While typically modest in magnitude, presidential coattails are more sizable in some elections. In 18 of the 150 elections examined presidential coattails made more than a three-percentage-point difference to the Senate vote.

Of course, the ultimate "difference" is in affecting an election outcome. By the above estimate of coattail effects, coat-

tails seem to have made the difference between winning and losing in 12 cases (8% of total).⁹ In these 12 Senate elections, the winning candidate's margin of victory was less than the help he apparently received from presidential coattails. These 12 cases, the estimated effects of coattails, and the Senate election margins are presented in Table 2.

Not surprisingly, given the success of Republican presidential candidates in this period, each of the Senate candidates apparently owing his or her election to presidential coattails is a Republican. Most of these Senate victories attributable to coattails came in two years, in Nixon's 1972 landslide victory over McGovern and in Reagan's 1980 defeat of Carter. The four Senate seats won with Reagan's 1980 coattails were especially important. These gains gave Republicans a majority in the Senate for the first time since 1954, a majority they lost when these Senate seats were next contested in 1986. Given the magnitude of Reagan's 1984 landslide, it is

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Table 2. Presidential Coattail Effects on Senate Election Outcomes

Year	State	Winning Senate Candidate	Presidential Coattail Effect (%)	Winning Senate Vote (%)
1988	Florida	Mack (R)	2.0 (R)	50
1988	Wyoming	Wallop (R)	2.0 (R)	50
1984	Kentucky	McConnell (R)	1.9 (R)	50
1984	North Carolina	Helms (R)	2.2 (R)	52
1980	Arizona	Goldwater (R)	3.3 (R)	51
1980	Idaho	Symms (R)	4.0 (R)	50
1980	New Hampshire	Rudman (R)	3.1 (R)	52
1980	North Carolina	East (R)	.2 (R)	50
1972	Idaho	McClure (R)	3.8 (R)	53
1972	Nebraska	Curtis (R)	3.7 (R)	53
1972	Oklahoma	Bartlett (R)	4.6 (R)	52
1972	Virginia	Scott (R)	3.5 (R)	53

Note: The winning Senate vote is the percentage of the two-party vote for the winning candidate. The presidential coattail effects are calculated based on the estimated coattail effect of .18 per percentage point of the presidential vote (see Table 1) and a baseline of 50% of the presidential vote. The coattail effect = $.18 \times (\text{presidential vote\%} - 50\%)$.

somewhat surprising that his coattails were only long enough to carry two Republican Senate candidates into office. Most recently, Bush's coattails helped Mack win an open seat contest in Florida by a narrow margin and helped incumbent Wallop fend off an unexpectedly hard challenge in Wyoming. Not surprisingly, the close presidential race of 1976 apparently failed to swing a single Senate race one way or the other.

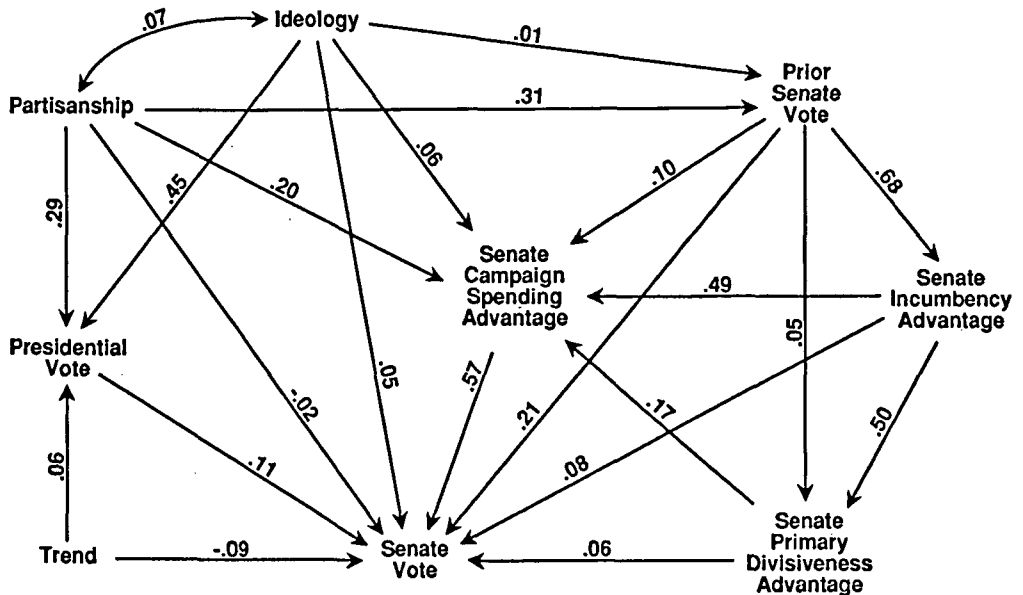
Another way of assessing coattail effects in Senate elections is to compare them to coattail influences in House contests. As might be expected, given the prominence of Senate elections, presidential coattails for Senate candidates are not as ample as those for candidates for the House. According to estimates of presidential coattails in House elections (Born 1984; Ferejohn and Fiorina 1985, 107), a ten-percentage-point increase in a party's presidential vote adds about four percentage points to its House vote. This suggests that presidential coattails for Senate candidates are about half as long as those provided to House candidates.

Common Causes

While presidential coattails link the Senate and presidential votes, the association appears to be more of a result of common cause. Recall that the bivariate estimate of presidential vote's association with the Senate coattails was .59 ($r = .37$) while the multivariate nonrecursive estimate of its actual effect is just .16 (beta = .10). This suggests that presidential coattails account for about one-quarter (27%) of the overall association between the two votes. Factors affecting both the presidential and Senate votes must account for the remaining association between the presidential and senatorial votes. The effects of these common causes can be assessed by the estimated paths of the full model presented in Figure 1.

Of these common causes, a common partisan electorate is most responsible for much of the association between the presidential and Senate votes beyond coattails. The general ideological composition of a state is related to the presidential vote but is only weakly related to the Senate vote.

Figure 1. Causal Model of Presidential Coattails and the Senate Vote



And while there is a minor partisan trend over this period favoring Republican Senate candidates, there is no such trend at the presidential level (the presidential shift may have taken place before 1972).

Only state partisanship seems to affect both votes significantly. Not surprisingly, Democratic presidential candidates tend to do better in Democratic states and less well in Republican states. In terms of the Senate vote a state's partisan composition affects previous Senate votes, and there is some continuity to that vote. Moreover, the stronger party in a state is more likely to have installed its candidate as an incumbent and may enjoy those advantages in future elections. Finally, the more popular party in a state generally finds it easier to raise more campaign funds than the opposition, and this campaign-spending advantage is the single strongest direct influence on the Senate vote.⁹

Conclusion

There are two major findings of this analysis. First, even in the recent period of weakened mass partisanship and the rise of more highly financed Senate contests, presidential candidates have coattails that affect the Senate vote. This concurs with the findings of Hinckley (1970), Key (1958), and Stewart (1987). While the impact of presidential coattails is not dominant, it is significant and in a number of elections may have been decisive. The fact that presidential coattails are significant in a time of dealignment and for an office whose candidates and campaigns have perhaps made it least likely to be subject to presidential coattail influence testifies to the power of presidential candidates to organize and to influence the electorate beyond its vote for the presidency alone. Second—despite partisan dealignment

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and the rise in ticket splitting—the partisanship of a state's electorate still makes a difference in both presidential and Senate contests. A good deal of the association between the Senate and presidential votes is a result of both votes coming from the same partisan electorate. Even in its weakened state, partisanship apparently still provides a strong cue for many voters deciding how to cast their Senate as well as their presidential votes.

Apart from affecting the immediate Senate election results, presidential coattails may have other consequences. They may influence future Senate elections. All things being equal, some portion of the Senate vote resulting from presidential coattails is likely to be lost when the seat is contested six years later in a midterm election, when those coattails are no longer available. Of course, as the effects of the prior Senate vote in the model suggest, Senate candidates may not lose all of the prior presidential coattail vote. Once voters have voted for a candidate, that candidate stands a good chance of holding their support and generally running a stronger campaign in the next election. If the direct and indirect effects of the prior Senate vote on the subsequent Senate vote in presidential years are any indication, Senate candidates should be able to hold more than half of their presidential coattail votes in their next election.¹⁰

Coattails may also affect Senate support for presidential proposals. From one perspective, senators in states carried by the winning presidential candidate may listen more closely to the president because their constituents, by their votes, have indicated their approval of the president. But beyond a desire to represent constituents, Senate candidates receiving coattail votes may be a bit more positively disposed, out of gratitude, to side with a president who had helped in their election. And even if modest in magnitude, coattail help, unlike state partisanship, is not a constant that can be taken for

granted. Moreover, in addition to influencing the perspectives of some senators, coattails may on occasion determine which party wins the Senate seat; and this certainly has an impact on Senate decision making (Bullock and Brady 1983).

Of course, it is also possible that coattails are inconsequential in persuading senators to support the president. Presidential coattail effects are fairly modest and therefore may not buy much support. Also, Senate candidates can benefit from coattails whether presidential candidates do anything actively on their behalf or not. Thus, if senators regard coattails as an unintended spillover effect of the presidential candidate's personal campaign, they may not think presidential candidates deserve any extra credit. Finally, given the sequence of elections and limits on presidential terms, senators can be ingrates without fear of presidential reprisal in future elections.

Notes

1. Abramowitz (1988) did include public assessments of "party competence." In that analysis, the party competence measure may have accounted for some part of the Senate vote variance actually explained by presidential coattails.

2. The presidential vote percentages are from Scammon and McGillivray (1988). The Senate vote percentages are from Erenhalt 1983, 1985, and 1987 and Congressional Quarterly's weekly reports.

3. Short-term forces are also a possible common cause of the presidential and Senate votes. Wright and Berkman (1986) have shown that issue considerations do affect the Senate vote and these same issues may enter into the presidential vote. Presumably, partisanship and ideology effects incorporate a substantial portion of these issue effects, at least insofar as they affect both presidential and senatorial candidates. The state of the economy was also considered as a possible common cause (Fair 1988; Hibbing and Alford 1982; Radcliff 1988). Initial analysis of pre-1988 economic data, however, indicated that the inclusion of economic effects did not appreciably alter the regression estimates. It may be that economic effects are more national in nature and are filtered through reactions to the presidential candidates.

4. As in the original Wright, Erikson, and McIver

(1985) study, both partisanship and ideological scores for Nevada are adjusted to the more Republican and conservative simulated scores. See n. 5 in that study for the rationale and procedures involved in the adjustment.

5. Seats that were vacated since the last election and filled by appointment are coded as open seats, whether the appointed senator was seeking election to the seat or not. While in some respects an appointed senator has incumbent advantages of an elected senator, in other respects he does not. Most importantly, unlike the elected senator, he has not demonstrated to voters, contributors, and potential opponents that he can win the seat in an election.

6. Campaign spending data from 1972 to 1984 are extracted from *The Almanac of American Politics* (Barone and Ujifusa 1981; Barone, Ujifusa, and Matthews 1975, 1979) and Ehrenhalt 1983, 1985, 1987. Data for 1988 are directly from a U.S. Federal Election Commission press release, "\$458 Million Spent by 1988 Congressional Campaigns," 24 February 1989. Examining only the effects of campaign spending, the comparative spending measure accounted for more variance in the Senate vote (adj. $R^2 = .61$) than the natural log of both Democratic and Republican spending (adj. $R^2 = .54$).

7. The overall fit of the regression seems quite good. Insofar as the R^2 indicates goodness of fit, the Senate vote equation fits about as well as Abramowitz' equations. His open seat equation explained 55% of the variance (std. error = 7.4) and his incumbency equation explained 75% of the variance (std. error = 5.0). These figures are only suggestive and are not directly comparable since the analysis was structured differently (e.g., in terms of the incumbent's party) and covers both Senate elections held in midterms as well as presidential election years. Also, the Abramowitz analysis includes several significant but fairly subjective variables like scandal, controversy, and health problems.

8. Coattail effects are qualified by *seem* to because they are point estimates around which one may want to draw confidence intervals. Also, a second baseline of the normal vote was considered. However, since the effects of state partisanship or its normal vote have been included in the general model (Figure 1), using the normal vote as a baseline would seem to be controlling twice its effects.

9. The effect of the campaign spending advantage on the Senate vote may be a bit misleading. Undoubtedly, a financial advantage of one candidate over his opponent, if well exploited, yields some votes for the better financed candidate. However, some part of the effects attributed to the campaign spending advantage reflect the comparative qualities of the candidates themselves rather than the impact of money per se (Green and Krasno 1988, 889).

10. The unstandardized direct and indirect effects of the prior Senate vote on the subsequent vote is about .65. This indicates that 65% of the prior vote is carried over to the next election. Of course, this ef-

fect may be different for votes received on the basis of presidential coattails rather than partisanship, ideology, and the voters' past assessment of the incumbent. We might expect greater attrition of coattail votes. Also, presidential home state advantage and incumbency effects on the presidential vote were specified in Figure 1; but since neither influenced the Senate vote, they are not shown. Both were significant.

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SENATE VOTING ON SUPREME COURT NOMINEES: A NEOINSTITUTIONAL MODEL

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We develop and test a neoinstitutional model of Senate roll call voting on nominees to the Supreme Court. The statistical model assumes that Senators examine the characteristics of nominees and use their roll call votes to establish an electorally attractive position on the nominees. The model is tested with probit estimates on the 2,054 confirmation votes from Earl Warren to Anthony Kennedy. The model performs remarkably well in predicting the individual votes of Senators to confirm or reject nominees. Senators routinely vote to confirm nominees who are perceived as well qualified and ideologically proximate to Senators' constituents. When nominees are less well qualified and are relatively distant, however, Senators' votes depend to a large degree on the political environment, especially the status of the president.

Roll call voting in the U.S. Senate on nominees to the Supreme Court presents political scientists with an empirical puzzle and a theoretical challenge. The empirical puzzle stems from a curious pattern in the nomination politics of recent decades. In some cases, as shown in Table 1, the Senate routinely confirms the nominee. In these cases, liberal senators vote for conservative nominees and conservative senators vote for liberal nominees. For example, the most liberal members of the Senate recently voted to confirm judicial conservative Antonin Scalia. But on other occasions—including 9 of the 20 post-*Brown-v.-Board of Education* confirmations (see Songer 1979)—the confirmation becomes extremely contentious. In these cases many or even most senators vote against the nominee, and voting becomes

ideologically polarized. The recent rejection of Robert Bork illustrates this case.

We therefore face some puzzling questions: Why are some votes consensual? Why are some votes contentious? And what determines voting decisions in both cases? Satisfactory answers to these questions must explain the apparent switching process between the consensual and conflictual votes and the variance within the conflictual votes.

Satisfactory analysis of roll call voting on Supreme Court nominees must do more than solve an empirical puzzle, however. It must also respond to the theoretical challenge raised by the "new institutional" approach to congressional policy making (McCubbins and Sullivan 1987).

The political science literature abounds with historical studies (Abraham 1974;

Table 1. Nominee Margin and Vote Status

Nominee	Year	President's Status ^a	Margin	Vote ^b
Warren	1954	strong	96-0 ^c	consensual
Harlan	1955	weak	71-11	conflictual
Brennan	1957	weak	95-0 ^c	consensual
Whittaker	1957	weak	96-0	consensual
Stewart	1959	weak	70-17	conflictual
White	1962	strong	100-0 ^c	consensual
Goldberg	1962	strong	100-0 ^c	consensual
Fortas 1	1965	strong	100-0 ^c	consensual
Marshall	1967	strong	69-11	conflictual
Fortas 2	1968	weak	45-43 ^d	conflictual
Burger	1969	weak	74-3	consensual
Haynsworth	1969	weak	45-55	conflictual
Carswell	1970	weak	45-51	conflictual
Blackmun	1970	weak	94-0	consensual
Powell	1971	weak	89-1	consensual
Rehnquist 1	1971	weak	68-26	conflictual
Stevens	1975	weak	98-0	consensual
O'Connor	1981	strong	99-0	consensual
Rehnquist 2	1986	strong	65-33	conflictual
Scalia	1986	strong	98-0	consensual
Bork	1987	weak	42-58	conflictual
Kennedy	1988	weak	97-0	consensual

^aThe president is labeled "strong" in a non-election year in which the president's party controls the Senate and "weak" otherwise.

^bA vote is labeled "conflictual" when less than 90% of the votes cast are cast on the winning side and "consensual" otherwise.

^cVoice vote.

^dVote on cloture—failed to receive necessary two-thirds majority.

Blaustein and Mersky 1978; Cole 1934; Danelski 1964; Friedman 1983; Grossman and Wasby 1972; Harris 1953; McHargue 1949; Schmidhauser 1979; Scigliano 1971; and Warren 1923) and statistical studies (Palmer 1983; Segal 1987) of Senate confirmation of Supreme Court justices. Few studies, however, address the puzzle of confirmation voting directly in terms of the roll call votes of individual senators over many confirmation votes. In fact, only three studies begin to address the puzzle at all (Felice and Weisberg 1988-89; Rohde and Spaeth 1976; Songer 1979). These studies all find evidence that the ideology of *senators* plays a role in controversial votes. By running separate models for each nominee, however, they

are unable to determine to what extent the ideology or qualifications of the *nominee* affects opposition. Further, because they restrict their attention to controversial votes, they cannot explain why some votes are controversial while others are not. Songer's declaration remains true: "There have been no systematic explanations why a majority of nominations remain essentially noncontroversial" (1979, 929). Moreover, no analysis of confirmation voting has yet employed insights from the new institutionalism. In short, the empirical puzzle remains a mystery and the theoretical challenge has been left unanswered.

First, we discuss institutional issues in confirmation voting. Second, based on

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this analysis, we develop a spatial model of roll call voting on confirmations. Using newly generated data on nominees, we then test the theoretical model.

A Neoinstitutional Perspective on Confirmation Voting

A neoinstitutionalist perspective suggests that votes on Supreme Court nominations depend on (1) the goals the senator pursues during the confirmation process, (2) the choices confronting the senator at each stage during the sequence of votes (or "agenda") leading to a filled vacancy on the Court, (3) the foresight the senator exercises in moving from one stage to the next in a multistage agenda, and (4) the payoffs the senator receives as a consequence of his or her choices (for related references, see Krehbiel 1988).

With respect to motivation, we imagine senators asking themselves, "Can I use my actions during the confirmation process to gain electoral advantage? Or if I am forced to account for my votes, can they be used against me? What is the most electorally expedient action for me to have taken?" Hence, we follow Mayhew (1974), Fenno (1978), and Fiorina (1974) in analyzing how the prospect of explaining behavior in Washington influences the behavior of representatives. We recognize (as did Mayhew, Fenno, and Fiorina) that senators often have additional goals in mind as they make highly visible decisions. Among these goals may be furthering a vision of good public policy and enhancing power and prestige within the Senate (Dodd 1985; Fenno 1973; Kingdon 1981). But a narrower focus on the electoral connection often captures much of the motivation of senators, provides a useful base line for more complex models, and offers an attractive, direct path to the statistical analysis of confirmation roll call voting.

In light of the electoral connection, a

senator is likely to view roll call voting as an opportunity for position taking and credit claiming. The senator can generally expect to gain electorally (or at least not to lose electorally) from voting as constituents wish and can expect to incur losses from flouting constituents' desires, regardless of the actual outcome of a vote. In addition, to the extent that the senator's vote actually sways the outcome, the senator may claim credit for a good outcome or receive blame for a bad one (Mayhew 1974; Weaver 1986).

Position taking and credit claiming must take place within the agenda of voting opportunities offered during the confirmation process. This agenda operates fairly simply: First, the president nominates a candidate; the Senate then votes on this candidate. If the nominee is approved, the process ends. If the nominee is rejected, the president nominates another candidate and the Senate votes on this candidate. The process continues until the seat is filled. In the twentieth century, the process has never proceeded past three votes and rarely past two.

As this agenda involves a potential series of votes, strategic voting is a possibility. For example, in an early stage of the agenda a senator might vote against a nominee constituents actually favor in order to create the opportunity to vote for an even better nominee at a later stage. Or a senator might vote for a nominee constituents oppose to block the confirmation of an even worse subsequent nominee.

Because strategic voting requires misrepresenting one's true motivation, it can present difficulties for senators compelled to explain roll call votes to their electorate. Under most circumstances, better chances for position taking and credit claiming result from what Denzau, Riker, and Shepsle (1985) call rationally nonstrategic behavior, that is, apparently sincere voting. We have suggested elsewhere (Cameron, Cover, and Segal 1989) that rationally nonstrategic voting is

almost always a better choice for senators than strategic voting when casting roll call votes on nominees to the Supreme Court.

The presence of strategic voting presents major problems for regression-based approaches to roll call voting (Krehbiel and Rivers 1985, 1988). On the other hand, rationally nonstrategic voting is observationally indistinguishable from sincere voting. A statistical analysis of roll call voting in confirmation politics can therefore proceed in a straightforward way.

A Spatial Model of Confirmation Voting

What factors would electorally minded senators care about when judging the merits of Supreme Court nominees? Both the public record and common sense suggest that the public's principal concerns in nomination politics (whenever that concern becomes manifest) are the characteristics of nominees. Two characteristics in particular receive close scrutiny in hearings and in the press: (1) the nominee's professional competence and (2) the nominee's judicial philosophy. The importance of high qualifications is exemplified by the universal ridicule heaped on Senator Roman Hruska's defense of Judge G. Harold Carswell's manifest mediocrity: "Even if he [Carswell] were mediocre, there are a lot of mediocre judges and people and lawyers. They are entitled to a little representation, aren't they, and a little chance" (Baum 1981, 47). The importance of judicial philosophy is suggested by the attacks by liberals on judicial conservative Robert Bork and those by conservatives on judicial liberals Thurgood Marshall and Abe Fortas. Ideologically proximate nominees will be attractive, poorly qualified nominees unattractive, and nominees who are both ideologically distant and poorly qualified very unattractive.

In addition, the president is not a passive bystander but an active participant in the nomination process. The president has nominated a particular person to satisfy his own constituents and possibly to further his own policy objectives. Failure to send the confirmation through the Senate harms the president's prestige, his reputation for competence, and possibly his popularity and his ability to govern (Neustadt 1960; Ostrom and Simon 1985). Therefore, the president is likely to bring his political resources to bear to help his nominee. In general, the president will have more political resources to deploy—and can deploy his political resources more effectively—when his party controls the Senate and when he is not in the fourth year of his term. In addition, presidential resources are not likely to affect every senator the same way. First, the president's resources will probably carry greater impact on members of his own party. Second, presidential resources are much more likely to change a senator's votes when the senator is more or less undecided on the basis of the nominee's characteristics.

Ideology can be considered a spatial characteristic; nominee qualifications and the strength of the president are nonspatial or valence characteristics of a nominee; party status is a nonspatial characteristic of a senator. Hence, define five variables, I_j , X_{ij} , U_j , S_j , and P_{ij} . I_j is a measure of nominee j 's ideology, ranging from zero (a very conservative nominee) to one (a very liberal nominee). X_{ij} measures the ideal or desired ideology for nominees held by senator i 's constituents at the time of nominee j , as perceived by senator i . U_j measures nominee j 's (lack of) qualifications, ranging from zero (highly qualified) to one (poorly qualified). S_j and P_{ij} measure, respectively, whether the president is strong at the time of nominee j (i.e., whether the president's party controls the Senate in a nonelection year) and whether senator i is from the

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same party as the president at the time of nominee j .

Slightly modifying a well-known spatial model incorporating both spatial distance and valence dimensions (Enelow and Hinich 1982; Enelow and Hinich 1984, secs. 5.1-2), senator i votes for nominee j if

$$U_j + \|I_j - X_{ij}\|^2 + U_{j*}\|I_j - X_{ij}\|^2 - S_j - P_{ij} < D_{ij*}, \quad (1)$$

where D_{ij*} is the "critical distance" or value separating acceptable from unacceptable nominees and $\|I_j - X_{ij}\|^2$ is the simple squared Euclidean distance, in this case just $(I_j - X_{ij})^2$. (See Cameron, Cover, and Segal 1989 for formal definitions of "acceptable" and "unacceptable" nominees.) The inclusion of an interaction term between distance and lack of qualifications, suggesting that nominees who are both ideologically distant and poorly qualified are very unattractive to senators, follows familiar precedent in spatial models (Enelow, Hinich, and Mendell 1986).

Assume the critical distance for senator i , D_{ij*} has two components, namely, $D_{ij*} = \delta^* + \epsilon_{ij}$ where δ^* is a distance or value common to all senators over all nominations and ϵ_{ij} is an unobservable distance specific to each senator for each nomination. Then equation 1 can be rewritten as

$$U_j + \|I_j - X_{ij}\|^2 + U_{j*}\|I_j - X_{ij}\|^2 - S_j - P_{ij} - \delta^* < \epsilon_{ij}. \quad (2)$$

If ϵ_{ij} is normally distributed with zero mean and variance σ^2 , equation 2 may be estimated as a probit equation, to wit,

$$P(V_{ij} = 1) = \Phi(\alpha_0 + \alpha_1 U_j + \alpha_2 D_{ij} + \alpha_3 U_{j*} D_{ij} + \alpha_4 S_j + \alpha_5 P_{ij}),$$

where $D_{ij} = \|I_j - X_{ij}\|^2$ and Φ is the cumulative normal probability density function.

Presidential influence may appear to

enter the model linearly; but because the probit model is intrinsically nonlinear, presidential influence has a much greater impact on senators near $P(V_{ij} = 1) = .5$ than $= .0$ or 1.0 , as indicated earlier.

Data

The dependent variable consists of the 2,054 confirmation votes cast by senators from the nomination of Earl Warren to the nomination of Anthony Kennedy.

To determine perceptions of nominees' qualifications and judicial philosophy we conducted a content analysis from a source that contains comparable information on each nominee since Earl Warren, statements from newspaper editorials from the time of the nomination by the president until the vote by the Senate. We selected four of the nation's leading papers, two with a liberal stance (the *New York Times* and the *Washington Post*) and two with a more conservative outlook (the *Chicago Tribune* and the *Los Angeles Times*). We note here that the data are reliable and appear to be valid.¹ Table 2 displays the ideology and qualification scores for the nominees from Earl Warren to Anthony Kennedy.

The spatial model requires a measure of senators' ideal points. Moreover, if the distance metric is to be meaningful, this measure must be comparable with that of the nominees. In the absence of a direct measure of the perceived preference of senators' constituencies about nominees' judicial philosophy, we have developed an inferential measure using senators' liberalism ratings calculated by the Americans for Democratic Action (ADA). The method we employ is based on the theory of predictive mappings in the spatial theory of voting and has been discussed at some length elsewhere (Cameron, Cover, and Segal 1989).²

We define a dummy variable S

("strong") which takes the value one when the president's party controls the Senate and the president is not in the fourth year of his term, zero otherwise. We define a dummy variable *P* ("party") which takes the value one when a senator is of the same party as the president, zero otherwise.

Table 3 provides summary statistics on the variables used in the model.

Table 2. Nominee Ideology and Qualification Scores

Nominee	Ideology	Qualifications
Warren	.75	.74
Harlan	.88	.86
Brennan	1.00	1.00
Whittaker	.50	1.00
Stewart	.75	1.00
White	.50	.50
Goldberg	.75	.92
Fortas 1	1.00	1.00
Marshall	1.00	.84
Fortas 2	.85	.64
Burger	.12	.96
Haynsworth	.16	.34
Carswell	.04	.11
Blackmun	.12	.97
Powell	.17	1.00
Rehnquist 1	.05	.89
Stevens	.25	.96
O'Connor	.48	1.00
Rehnquist 2	.05	.40
Scalia	.00	1.00
Bork	.10	.79
Kennedy	.37	.89

Note: .00 is the most conservative and 1.00 the most liberal score possible. .00 is the least qualified and 1.00 the most qualified score possible.

Results

The maximum likelihood estimates of the coefficients in our model may be summarized succinctly:³

$$\begin{aligned}
 P(V_{ij} = 1) = & \Phi [1.80 - 1.20U_i \\
 & (15.31) \quad (-4.44) \\
 & - 1.19D_{ij} - 9.97U_i \cdot D_{ij} \\
 & (-4.50) \quad (-9.80) \\
 & + 1.39S_j + .74P_{ij}]^3 \quad (3) \\
 & (8.97) \quad (6.50)
 \end{aligned}$$

(MLE's/SE's are in parentheses; $\chi^2 = 985.9$; estimated $R^2 = .746$; percentage correct = 93.0; and percentage improvement = 53.5.)

As the estimates in equation 3 indicate, the overall fit of the model is extremely good: the χ^2 is significant at $p < .001$; the proportion of votes explained correctly by the probit equation is 93%; the pseudo- R^2 for the equation is approximately .75; and the percentage improvement in predicted votes over the margin is a little over 53%. All of the estimated coefficients take the predicted sign, are significant at $p < .001$ or greater, and are of reasonable magnitudes.

As the results indicate, confirmation voting is decisively affected by the ideological distance between senators and nominees. Equally important, as indicated by the virtually identical parameter estimates on the *U* and *D* terms, are the qualifications of the nominee. Overwhelmingly, however, it is the interaction

Table 3. Dependent and Independent Variables

Variable	Mean	Variance	Minimum	Maximum
Vote	.850	.123	.00	1.00
Squared Euclidean distance (D)	.243	.070	.00	1.00
Lack of qualifications, 1 - Q (U)	.195	.063	.00	.89
Strong president (S)	.372	.234	.00	1.00
Same party status (P)	.500	.250	.00	1.00
Distance $\times$ lack of qualifications (UD)	.045	.011	.00	.89

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Table 4. Examples of Voting Probabilities

Ideological distance	Lack of Qualification (0 = most qualified, 1 = most unqualified)										
	.0	.1	.2	.3	.4	.5	.6	.7	.8	.9	1.0
.0	.96	.95	.94	.93	.91	.88	.86	.83	.80	.76	.73
.1	.95	.93	.89	.85	.79	.72	.64	.56	.47	.38	.30
.2	.94	.89	.82	.73	.61	.52	.36	.26	.14	.10	.05
.3	.93	.85	.73	.58	.41	.26	.14	.07	.03	.01	—
.4	.91	.79	.61	.41	.23	.10	.04	.01	—	—	—
.5	.89	.72	.51	.26	.10	.03	.01	—	—	—	—
.6	.86	.64	.36	.14	.04	.01	—	—	—	—	—
.7	.83	.56	.26	.07	.01	—	—	—	—	—	—
.8	.80	.47	.14	.03	—	—	—	—	—	—	—
.9	.77	.39	.10	.01	—	—	—	—	—	—	—
1.0	.73	.31	.05	—	—	—	—	—	—	—	—

Note: Examples assume a weak president and a Senator not of the president's party.

of qualifications and ideology that determines the votes of senators.

As probit estimates are not readily interpretable in terms of probabilities, we provide in Table 4 examples of voting probabilities for varying levels of ideological distance and qualifications. Senators, even opposition senators serving with a weak president, will vote for a poorly qualified nominee if the nominee is ideologically close (e.g., Southern Democrats for Clement Haynsworth). They will vote for an ideologically distant nominee if the nominee is highly qualified (e.g., liberal Democrats for Anthony Kennedy). Ideological distance, however, becomes paramount for nominees with even moderate questions concerning their qualifications. Alternatively, we could say that qualifications become paramount for nominees of even moderate ideological distance from senators.

Additionally, presidential influence and same party status have a powerful impact on voting probabilities, especially for senators who remain undecided after examining the characteristics of the nominee. For example, in the model a switch from a weak to a strong president raises to

.92 the probability of a *yes* vote from a senator who was previously undecided. Similarly, same party status raises the probability of a *yes* vote from .5 to .78.

In order to solve the puzzle of confirmation voting, a model must not only explain individual votes but also correctly distinguish consensual from conflictual votes. In the sample of 22 votes, 13 are consensual and 9 are conflictual. As shown in Table 5, the model correctly identifies all of the consensual votes. It is almost as successful in identifying the conflictual votes; 7 of the 9 conflictual votes are correctly identified (78%). It fails to identify the Stewart and Marshall confirmations as conflictual; in these confirmations a group of conservative southern senators voted against the nominees. The model fails to capture the source of the senators' (apparently race-related) opposition.

The model must also account for variance in voting. A useful summary measure of the success of the model in accounting for the variation in the votes is mean absolute error. On average the model misidentifies only five votes per confirmation. Within the consensual

**Table 5. Actual Versus
Predicted No Votes**

Nominee	Actual	Predicted
Warren	0	0
Harlan	11	25
Brennan	0	0
Whittaker	0	0
Stewart	17	0
White	0	0
Goldberg	0	0
Fortas 1	0	0
Marshall	11	0
Fortas 2	43	36
Burger	3	0
Haynsworth	55	46
Carswell	51	53
Blackmun	0	0
Powell	1	0
Rehnquist 1	26	13
Stevens	0	0
O'Connor	0	0
Rehnquist 2	33	35
Scalia	0	0
Bork	58	29
Kennedy	0	0

Note: Mean absolute error for all votes = 4.9; for consensual votes = .2; for conflictual votes = 11.6; r actual versus predicted = .91.

votes, however, the mean absolute error is almost zero. Within the conflictual votes, the mean absolute error is higher—11–12 votes. The correlation between actual and predicted *no* votes is .91.⁴

Gauging the success of the model in terms of confirmation outcomes is not a straightforward task. The vote on the Fortas nomination as chief justice was 45 *yea* and 43 *nay*, but this majority was insufficient to invoke cloture, so the nomination was defeated. Even under current rules, it is reasonable to presume that no nominee will pass with more than the 43 “nays” received by Fortas. (In fact, no nominee this century has been confirmed with more than 33 negative votes.) If we use 43 as the number of negative votes needed to ensure defeat, the model correctly predicts the outcome of every nomination except Bork’s and Fortas’.

Conclusion

The model resolves the puzzle of confirmation voting straightforwardly. When a strong president nominates a highly qualified, ideologically moderate candidate, the nominee passes the Senate in a lopsided, consensual vote. Presidents have often nominated this type of candidate and consequently consensual votes have been fairly common. When presidents nominate a less well qualified, ideologically extreme candidate, especially when the president is in a weak position, then a conflictual vote is likely. Surprisingly, presidents have nominated quite a few candidates of this description, and conflictual votes occur periodically. In short, the behavior of senators emerges as sensible, predictable, and readily understandable; the real source of the puzzle in confirmation voting appears to be the behavior of presidents in choosing nominees.

Learning more about confirmation voting requires moving beyond our simple model of position taking. First, the framework assumes senators are “single-minded seekers of reelection,” but we know this is not so. A more appealing framework would allow senators to trade off among competing goals in their roll call votes. In particular, to what extent do senators follow the (presumptive) desires of their constituents in confirmation votes, and to what extent do they “shirk” by voting their personal preferences? Second, future research might find it worthwhile to examine the role of interest groups in the nomination process. Finally, much more attention needs to be given to presidential selection of nominees.

In essence, we are suggesting the need to take a much more inclusive view of the nomination process. But an important issue then comes to the fore: What is properly exogenous and what is properly endogenous in the theory and models? For example, we treat the nominees’ ideolo-

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gies and qualifications as exogenous, but presidents presumably pick their nominees with the Senate's composition at least partly in mind. In addition, public perceptions of quality and ideology could be regarded as endogenous if interested parties can affect those perceptions. And any attempt to model interest group mobilization must confront the fact that their mobilization results from calculations by the groups and therefore must be regarded as endogenous as well. Solving these problems is likely to be the major challenge facing future analysts of confirmation voting and nomination politics more generally.

Notes

We thank Renee Adwar, Yen Giang, and Regina Stephanie Good for research assistance. James M. Enelow provided many helpful and timely suggestions. This material is based upon research supported by the National Science Foundation under grant no. SES-8812935.

1. We have discussed details of the content analysis elsewhere (Cameron, Cover, and Segal 1989). The measures of ideology and qualification pass standard criteria for intercoder reliability; using π as our index (see Krippendorff 1980), results of .72 for ideology and .87 for qualifications were achieved. Both figures are significant at $p < .001$. The ideology measure also passes the most stringent measure for validity, predictive validity. For confirmed nominees this measure is highly correlated with later votes on the Court dealing with civil rights and civil liberties (Segal and Cover 1989).

2. Tests reported in Cameron, Cover, and Segal 1989 indicate that the ideal points of senators can be measured on the same metric as nominee ideology scores. Our estimates are virtually unchanged if, following Wright and Berkman (1986), we use predicted, rather than actual, ADA scores. See n. 3.

3. Using predicted rather than actual ADA scores to determine D_{ij} yields

$$P(V_{ij} = 1) = \Phi \left[\begin{array}{ccc} 1.56 & -1.67U_j & -1.20D_{ij} \\ (16.39) & (-7.84) & (-4.19) \\ -5.85U_j \cdot D_{ij} & +.99S_j & +.42P_{ij} \\ (-6.42) & (8.65) & (4.55) \end{array} \right]$$

4. The model does not include dummy variables for individual nominees. Inclusion of such dummy variables drives the mean absolute errors to very low levels even in the conflictual votes and con-

siderably improves prediction of outcomes. However, the substantive interpretation of these "black box" factors is not very clear and prevents predicting out of sample.

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BLACK AND HISPANIC SOCIOECONOMIC AND POLITICAL COMPETITION

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Many U.S. cities are becoming significantly multiminority. How does the significant presence of one minority group affect the other minority group? This research explores the question of socioeconomic and political competition between blacks and Hispanics in U.S. urban centers. Based on data from the 49 U.S. cities of over 25,000 population with at least 10 percent black and 10 percent Hispanic in 1980, findings indicate that while there is little evidence of general black and Hispanic socioeconomic and political competition, Hispanics appear to prosper less well socioeconomically and politically in cities with black majorities or pluralities.

In recent years, the urban racial landscape has been profoundly transformed. Many U.S. cities have become significantly multiminority, with large concentrations of blacks and Hispanics in particular (Weaver 1982). While much urban research has focused on the representation of blacks and Hispanics in local government (e.g., Brouthers and McClure 1985; Campbell and Feagin 1975; Cole 1974; Eisinger 1982; Fraga, Meier, and England 1986; Garcia 1986; Karnig 1976, 1979a, 1979b; Karnig and Welch 1980; MacManus and Cassell 1982; Preston 1983; Robinson and Dye 1987; Welch, Comer, and Steinman 1973, 1975), few investigations have examined the relationship between blacks and Hispanics in their struggles for political power and economic advancement (Falcon 1988). In this research note, we explore the question of socioeconomic and political competition between blacks and Hispanics in U.S. cities. Does the significant presence of one minority group

affect the other minority group? Are political and economic outcomes complementary or do the successes of one minority come at the expense of the other?

Competition

Blalock (1967) suggested that group competition accounts for some aspects of discrimination experienced by minorities. As defined by Blalock, competition exists when two or more groups strive for the same finite objectives, so that the success of one may imply a reduced probability that another will attain its goals. According to Blalock, one could view group competition in terms of power contests which exist when there is rivalry and groups have roots in different cultures. Although addressed to majority-minority relations, Blalock's framework also is useful in examining relationships between minority groups if one recognizes that not only status differences but status similari-

ties may become bases for conflict.

In the 1960s, poverty promoted alliances between blacks and Hispanics, especially Mexican-Americans (Estrada et al. 1981), and there is clear evidence of coalition building between blacks and Hispanics (see, e.g., Browning, Marshall, and Tabb 1984). However, policies designed to promote equal access and equity for different groups were sometimes in conflict; Falcon, for example, notes that blacks were concerned with desegregation and were not supportive of bilingual education because they feared it would divert resources from black concerns (1988, 178). Moreover, Hispanics began to question whether they had gained as much as blacks had from affirmative action and antipoverty programs, since blacks were perceived to have secured more public sector jobs than Hispanics had (Cohen 1982; Falcon 1988).

Recent research indicates that in many communities blacks and Hispanics compete for scarce jobs, adequate housing, and government services (Falcon 1988; MacManus and Cassell 1982; Oliver and Johnson 1984; Welch, Karnig, and Eribes 1983); and in other cities, such as Miami, blacks have not enjoyed the economic development benefiting whites and Hispanics (Warren, Stack, and Corbett 1986). Further, survey data from Los Angeles suggests a growing hostility and distrust between the two groups (Oliver and Johnson 1984), with a majority of Mexican-Americans not in favor of building coalitions with blacks (see also Ambrecht and Pachon 1974; Browning, Marshall, and Tabb 1984; Grebler, Moore, and Guzman 1970; Henry 1980). There is also evidence that Hispanics, by and large, do not vote for black candidates in municipal elections (Hahn and Almy 1971; Mohl 1982).

With blacks and Hispanics struggling to achieve political and socioeconomic objectives, there are several possible intergroup patterns. First, one group may do

well at the expense of the other. Second, the successes of each group may be independent but come at the expense of non-Hispanic whites. Third, either through coalitions or as a consequence of the aggregate effects of independent actions, black and Hispanic successes may covary positively except when the situation is zero-sum, for instance, in elections for mayor and city council, where the number of seats is discrete.

Data and Methods

Various measures of socioeconomic and political outcomes were employed in this study (see Karnig and McClain 1985). Socioeconomic indicators included median education,¹ median income, percent nonpoverty population, and percent employed. Political measures concerned elective municipal office: whether blacks (Hispanics) held mayoral offices, what percentage of the city council was black (Hispanic), and the proportionality of black (Hispanic) council representation when standardized by the size of the black (Hispanic) population in the city. Data were collected on all 49 U.S. cities over 25,000 containing at least 10 percent blacks and 10 percent Hispanics in 1980.² With respect to Hispanics, 19 cities located in the West and Southwest contained principally Mexican-American populations, Miami was predominantly Cuban, 12 cities in the Northwest were largely Puerto Rican, and six cities in the Midwest contained a mix of Hispanic populations. Simple and partial correlations were used to test our propositions.

Findings

In our 49 cities, whites constituted 51% of the populations, blacks 25%, and Hispanics 19%. (The remainder were Asian, American Indian, and others.) Sample averages parallel census data for the na-

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tion as a whole: Among the socioeconomic indicators, white income (\$20,375), nonpoverty (86%), and unemployment (94%) were the highest; black income (\$14,245), nonpoverty (74%), and employment (89%) were the lowest; and Hispanic outcomes were intermediate (\$15,374 income, 77% nonpoverty, 90% employment). Also congruent with national data is the finding that white median education (12.4) is the highest, followed in order by black (12.2) and then Hispanic education (10.1). With respect to political measures, whites have the most favorable outcomes, winning mayorships (76%) and council seats (74%) far in excess of their proportion of the population (1.58 proportionality). Blacks rank second on these representational scores (16% mayorships, 20% council seats, .77 proportionality). Hispanics are a distant third (6% mayorships, 6% council seats, and .26 proportionality).

The first column of Table 1 tests directly whether black and Hispanic successes are either at the expense of one another or at the expense of whites. For *socioeconomic* outcomes, the findings support the positive covariation model. Where any group prospers with respect to education, income, employment, and nonpoverty, the other groups do significantly better as well.

The *political* variables portray a somewhat different pattern. There is no significant relationship between black and Hispanic election to mayoral posts or council seats, although there is a mild positive link between the proportionality of their council representation. This evidence indicates neither widespread direct political competition nor prominent mutual support—although it is possible that both exist and simply cancel one another in the aggregate. However, there is evidence of political competition between Hispanics and whites and (particularly) between blacks and whites. Clearly, if blacks or Hispanics are to win elective seats, it must

come at the expense of whites, who already control approximately 75% of mayoral and council positions. In principle, there is no inherent reason for socioeconomic competition among the three groups, since socioeconomic outcomes may not be discrete. Indeed, a rising tide may lift all boats. Conversely, since there is only a limited number of elected positions—one mayor, 5–50 council posts—the victory of one group must come at the expense of some other.

The “rising tide” metaphor raises the question of spuriousness with respect to socioeconomic outcomes. At the start of the analysis, we thought it possible—actually, probable—that positive simple correlations between black and Hispanic socioeconomic measures would be due to differences in the opportunity structures of cities. For example, New York and Chicago residents tend to have higher salaries than their counterparts in Galveston or Waco. Hence, positive relationships would be effects of the opportunity structure. To test the possibility of such spuriousness, we controlled for the standard in each community, that is, outcomes for whites.

The final column of Table 1 displays socioeconomic findings controlling for white outcomes. Rather to our surprise, there is little evidence of spuriousness. While each partial relationship between black and Hispanic outcomes is lower than its matched simple correlation, all the partials are positive and statistically significant at $p < .01$. Decreases in the size of correlations once controls are introduced signal that economic opportunities are certainly important. Nonetheless, the consistent positive correlations, even with controls, suggest no deleterious inter-minority *socioeconomic* competition.

For obvious reasons, when white outcomes are controlled, there are strong and negative links between black and Hispanic *political* outcomes. On the one hand, the findings concerning mayorships

and council positions are mere statistical artifacts. Once the results for whites are controlled, the relationships between black and Hispanic victory must be -1.00 , since all remaining competition is between blacks and Hispanics. On the other hand, the statistical artifacts do

drive home a basic point that is often overlooked: Unless complex coalitional agreements are struck by blacks and Hispanics, as white control over mayoral posts and council seats diminishes, there may be inescapable competition between blacks and Hispanics. As long as the num-

**Table 1. Correlations between Black, Hispanic, and White
Socioeconomic and Political Outcomes**

Relationships	Simple Correlations	Partial Correlations
Socioeconomic		
Nonpoverty		
Black-Hispanic	.60****	—
Black-white	.50****	—
Hispanic-white	.44****	—
Black-Hispanic (controlling white)	—	.48****
Income		
Black-Hispanic	.53****	—
Black-white	.42****	—
Hispanic-white	.59****	—
Black-Hispanic (controlling white)	—	.39****
Education		
Black-Hispanic	.42****	—
Black-white	.23*	—
Hispanic-white	.45****	—***
Black-Hispanic (controlling white)	—	.36***
Employment		
Black-Hispanic	.86***	—
Black-white	.66****	—
Hispanic-white	.60****	—
Black-Hispanic (controlling white)	—	.77****
Political		
Mayors		
Black-Hispanic	-.11 n.s.	—
Black-white	-.82****	—
Hispanic-white	-.47****	—
Black-Hispanic (controlling white)	—	-1.00****
Percent council		
Black-Hispanic	.17 n.s.	—
Black-white	-.91****	—
Hispanic-white	-.56****	—
Black-Hispanic (controlling white)	—	-1.00****
Council proportionality		
Black-Hispanic	.31**	—
Black-white	-.43****	—
Hispanic-white	-.41***	—
Black-Hispanic (controlling white)	—	-.17 n.s.

* $p < .10$.

** $p < .05$.

*** $p < .01$.

**** $p < .001$.

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ber of elective political positions is finite, one group's victory must come at the cost of another's defeat. The overwhelming political dominance of whites now masks the competition that may follow in the future as white population majorities decline and both black and Hispanics seek to increase their rates of electoral success.

The evidence to this point is remarkably consistent: (1) black and Hispanic *socioeconomic* outcomes covary positively with one another and with white socioeconomic outcomes; and (2) black and Hispanic *political* outcomes are largely independent, although both are negatively related to white representation. But the question of minority group status is a relative one. In recent years, we have witnessed an increase in the number of cities with "minority majorities," that is, where blacks or Hispanics have become the population majority, or at least plurality. When such majority or plurality conditions arise, do minorities tend to act as majorities? Are the new majorities apt to take a disproportionate share of the "goodies"? We know from cities such as Newark and Atlanta, for example, that once the black population becomes a majority, it may then always win the mayoralship and may gain a higher share of council seats in each election than its percentage of the population.

Table 2 tests the proposition that an increasing proportion of one minority negatively affects the socioeconomic and political outcomes of the other. Simple correlations are reported between the percentage black (Hispanic) and the other group's outcomes. In addition, to reduce possible spuriousness, partial correlations, controlling for white outcomes, were computed. The findings are mixed. Increases in the Hispanic proportion of a city's population do not appear related to competition harmful to blacks. For instance, none of the zero-order correlations between the percentage Hispanic and black socioeconomic indicators was significant at $p <$

.05. Of the partials, only two were statistically significant. Moreover, one of these (impact on black nonpoverty) is not in the predicted direction, and the other (reduction of black representation on councils) is countervailed by the absence of any negative link to black council proportionality. It appears, then, that Hispanic population increases are not injurious to black socioeconomic and political development, though the presence of only two sample cities with Hispanic populations nearing or surpassing 50% makes firm generalization impossible.

Table 2 does, however, suggest possible negative effects of black population size on Hispanics, particularly in socioeconomic development. In the zero-order case, the percentage black is negatively and significantly associated with Hispanic nonpoverty ($r = -.41$), income ($r = -.32$), education ($r = -.41$), and employment ($r = -.24$). To be certain that these findings were not consequences of poverty, poor schools, and so on in communities with large black concentrations, we created four additional measures: black-Hispanic ratios of nonpoverty, income, education, and employment.³ Simple correlations indicate that as the black proportion of the population rises, there is a statistically significant increase in black income versus Hispanic income ($r = .41$), nonpoverty ($r = .30$), and education ($r = .38$) but no significant link with the black-Hispanic employment ratio.

When controls for white outcomes are established, the picture becomes a bit fuzzier but still suggests that increases in the black proportion of a city may tilt outcomes in favor of blacks. While the partials give no evidence of a significant effect on Hispanic education and employment, the relationships are in the direction predicted by the "majority" dominance hypothesis. More critically, on the key poverty and income measures, even with controls for white outcomes, increasing

Table 2. Correlations between Percent Black (Hispanic) and Socioeconomic and Political Outcome Measures, with and without Controls for White Outcomes

Outcomes	Percent Black	Controls for White Outcomes	Percent Hispanic	Controls for White Outcomes
Socioeconomic				
Nonpoverty				
Hispanic	-.41***	-.31**	—	—
Black	—	—	-.18 n.s.	-.28**
Black-Hispanic	.30**	-.32**	.08 n.s.	.09 n.s.
Income				
Hispanic	-.32**	-.13 n.s.	—	—
Black	—	—	-.03 n.s.	-.12 n.s.
Black-Hispanic	.41***	.36***	-.03 n.s.	-.11 n.s.
Education				
Hispanic	-.41***	-.17*	—	—
Black	—	—	-.21 n.s.	-.17 n.s.
Black-Hispanic	.38***	.13*	.07 n.s.	-.03 n.s.
Employment				
Hispanic	-.24**	-.06 n.s.	—	—
Black	—	—	-.04 n.s.	.07 n.s.
Black-Hispanic	-.13 n.s.	.10 n.s.	-.04 n.s.	.08 n.s.
Political				
Mayor				
Hispanic	-.11 n.s.	-.35***	—	—
Black	—	—	.05 n.s.	-.20 n.s.
Percent council				
Hispanic	-.12 n.s.	-.40***	—	—
Black	—	—	.18 n.s.	-.39*
Council proportionality				
Hispanic	-.16 n.s.	-.03 n.s.	—	—
Black	—	—	.11 n.s.	.21 n.s.

* $p < .01$.
 ** $p < .05$.
 *** $p < .10$.

black population size is related to more favorable outcomes for blacks vis-à-vis Hispanics. Specifically, with controls for white outcomes, the percentage black is significantly associated with the black-Hispanic nonpoverty ratio ($r = .32$) and the black-Hispanic income ratio ($r = .36$).

These tests suggest more favorable consequences for blacks when their population percentage increases. However, the analyses are not altogether appropriate to test the dominance proposition, which contends that when a group reaches majority or at least plurality status, it begins

to obtain a disproportionate share of favorable outcomes, especially with respect to other minorities. To more satisfactorily examine this hypothesis, the sample was partitioned into cities with and without a black majority or plurality. Only six cities qualified as having black majorities or pluralities, ranging from a minimum of over 40% in Chicago to a maximum of 74% in Compton, California. With so few majority or plurality cases, it was impossible to test for statistically significant differences. However, while providing only modest exploratory

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**Table 3. Socioeconomic and Political Outcomes in Cities
with and without Black Majorities or Pluralities**

Outcomes	Black Majority-or- Plurality Cities (n=6)	Black Minority Cities (n=43)
Socioeconomic		
Percent nonpoverty		
Hispanics	.67	.78
Blacks	.71	.74
Percent employment		
Hispanic	.87	.91
Black	.86	.90
Median income		
Hispanic	12,820	15,731
Black	13,858	14,300
Median years of education		
Hispanic	9.13	10.19
Black	12.28	12.13
Black-Hispanic ratio		
Income	1.12	.93
Education	1.31	1.20
Nonpoverty	1.09	.95
Employment	.99	.99
Political		
Mayor		
Hispanic	.00	.07
Black	1.00	.05
Percent council representation		
Hispanic	.003	.07
Black	.49	.16
Proportionality of council representation		
Hispanic	.02	.30
Black	.91	.76

information, it may be instructive to focus on the data displayed in Table 3.

Examination of the means indicates that on all but one measure Hispanics fare less well and that on all but three indicators blacks fare better, in cities with a black majority or plurality. For example, while some of the mean differences are small, of the 18 means presented, 14 (78%) are in the predicted directions. The exceptions are higher black nonpoverty, income, and employment rates in black minority cities, and the equality of the black-Hispanic employment ratios in both sets of cities. These results are likely due to the shortfall

of economic opportunities in black majority cities, Camden and Newark in particular. But even on this issue, the gap between black income in cities with a black minority and cities with a black majority or plurality is less than \$450, while the difference for Hispanics in the two kinds of cities is over \$2,900. On each of the other scores, blacks are favored where they are at least a plurality—gaining higher education and having substantially greater chances of winning the mayor's seat (100%–5%),⁴ more council seats, and nearly equitable council representation (.91).

In cities with black majorities or pluralities, Hispanics appear to be especially disadvantaged politically. For example, in cities with a black majority or plurality, Hispanics have not won a mayor's race; and they have gained an overall grand total of only one council seat. In addition, the proportionality of their council representation is a barely perceptible 2%, compared with (an already desultory) 30% in black minority cities. Furthermore, the black-Hispanic ratio measures, perhaps the most meaningful socioeconomic indicators, suggest sharp differences. In black minority cities, the black-Hispanic income, nonpoverty, education, and employment ratios respectively are .93, .95, 1.2, and .99. The matched ratios in cities with a black majority or plurality are 1.12, 1.09, 1.31, and .99. With the exception of the employment ratio, which is the same (.99) in both sets of cities, Hispanics evidently do comparatively less well in communities in which blacks are at least a plurality. Stated differently, Hispanics compared to blacks fare less well by 19% in income, 12% in the percentage not in poverty, and 11% in years of education.

Conclusion

The forgoing inquiry was exploratory. The various models were underspecified, and unique conditions—especially in the handful of cities with a black majority or plurality—may help account for the findings. More rigorous examination is needed before firm generalizations can be developed. Nevertheless, the preliminary research does suggest some interesting patterns concerning possible socioeconomic and political competition between blacks and Hispanics. Remember, we posited several possibilities: (1) one group may do well at the expense of the other; (2) the successes of each group may be independent of each other but at the ex-

pense of whites; and (3) the successes of blacks and Hispanics may covary positively.

In our initial analyses, the *socioeconomic* data revealed no harmful competition in general. Where one group does better socioeconomically, so do the other two groups. *Political* outcome data presented a somewhat different picture. When either blacks or Hispanics gain politically, they do so at the expense of whites. Political competition between blacks and Hispanics is evident only when controls for white political outcomes are introduced. This suggests that as black and Hispanic political successes increase, political competition between blacks and Hispanics may be triggered, especially as fewer whites reside in minority-dominated cities.

In addition, evidence also indicates that competition does appear to occur as the size of the black population increases—with consequences to Hispanics, particularly on several socioeconomic measures. Moreover, in the small sample of cities in which blacks are a plurality or majority, Hispanics seem to fare less well socioeconomically and, in particular, politically. These latter findings may have broad implications for Hispanic political representation. Research suggests that minority resources of one kind have implications for other types of resources (Karnig 1979a; Karnig and McClain 1985; Robinson, England, and Meier 1985). Diminished political representation may have negative consequences for socioeconomic outcomes, especially in public sector jobs; and without socioeconomic means, a minority may find it difficult to gain a foothold in the political arena. If Hispanics (or blacks) do not receive a fair share of economic resources, they may not have the wherewithal to increase their political representation.

These findings also may have implications for the formation of political coalitions between blacks and Hispanics. In

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cities in which both blacks and Hispanics are minorities, where competition for representation appears to be between minorities and whites, the desire to increase the minority share of favorable political outcomes may represent the basis for coalitions. However, in cities with a black majority or plurality, where Hispanics are likely to compete with blacks for representation, there may be less of a foundation upon which to build coalitions. Indeed, Hispanics may see opportunities in building coalitions with remaining non-Hispanic white groups.

Ethnic tension and competition is not unique to the United States. Historical and current instances are nearly limitless, for instance, Greek-Turk, Jew-Arab, Japanese-Korean, English-Irish, Albanian-Serb, Armenian-Azerbaijani, Russian-Ukrainian, and Spanish-Basque. Moreover, comparative experience suggests that ethnic competition may be muted as a result of intergroup cooperation when a dominant third power is present. However, when the dominant group loses influence or withdraws—as occurred with the departure of colonial powers—ethnic competition may ensue among the remaining groups, for instance, Ibo, Hausa, and Yoruba in Nigeria; African and Asian (as well as between various tribes) in Uganda; Chinese and Malay in Malaysia; and Hindu and Sikh (as well as Moslem) in India.⁵

Given the persistence of ethnic identification, cultural differences, and often distinctive features or surnames, future black-Hispanic intergroup competition may increase. Until at least well into the twenty-first century, blacks and Hispanics will remain minorities nationally; and they are likely in the foreseeable period to remain socioeconomically underdeveloped and politically underrepresented. Given these conditions, blacks and Hispanics are apt to view themselves as minorities even if they constitute population majorities in particular political juris-

dictions. It would be natural for each to view local majority status as an opportunity to compensate for deprivations elsewhere—at least, that is the comparative experience of majorities and minorities abroad. Hence, the new jurisdictional “majority” may begin to function as a “dominant majority”—receiving a disproportionate share of favorable outcomes—which may help to assure that the other “minority” is treated less equitably.

Notes

This is a revised version of a paper presented at the 1988 annual meeting of the American Political Science Association, Washington, DC. We thank Peter K. Eisinger and John A. Garcia for helpful comments on an earlier version, and Dongsung Kong, our research assistant, for data management.

1. The median-years-of-education variable was available for 43 of the cities; the remaining 6 were calculated from grouped data reported in the U.S. Bureau of Census 1980.

2. Information on socioeconomic indicators was taken from the U.S. Bureau of the Census 1980. Data on black elected officials were drawn from Joint Center for Political Studies 1987, and information on Hispanic elected officials was drawn from National Association of Latino Elected Officials 1985. Municipal Yearbooks of 1982–83 were the sources of data on city council size; phone calls were made to collect information on cities that were not listed.

3. The ratios were constructed by dividing Hispanic outcomes into black outcomes, for instance, black income divided by Hispanic income. When black and Hispanic outcomes are equal, the ratio is 1.0; when blacks do better, the ratio exceeds 1.0; where Hispanics are more successful, the ratio is below 1.0.

4. The research was undertaken before the 1989 election of Richard Daley, Jr. in Chicago—the only case in our sample in which a city with a black plurality or majority did not elect a black mayor. Even with this most recent exception counted, blacks won the mayorship in cities with a black majority or plurality a robust 83.3% of the time.

5. The examples are chosen because they will be easily recognized. There is no intention to suggest that competition between blacks and Hispanics will be violent.

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CONTROVERSIES

SHOULD MARKET FORCES CONTROL EDUCATIONAL DECISION MAKING?

In the December 1988 issue of this Review, John Chubb and Terry Moe presented data comparing public and private schools, and made an argument concerning "politics, markets, and the organization of schools." Chubb and Moe argue that private schools outperform public schools because they are more autonomous, advantaged by market forces rather than democratic political control. Jack Tweedie takes vigorous exception to this conclusion, arguing that the evidence does not support Chubb and Moe's conclusions about the efficacy of market forces. Dennis Riley directly attacks the virtues of market control of institutional choices in educational policy making. Chubb and Moe find their critics unconvincing.

In "Politics, Markets, and the Organization of Schools," John Chubb and Terry Moe (1988) examine institutional choice in education, specifically the issue of whether political control or market responsiveness results in better schools. They report the descriptive results of a survey of private and public schools' organizational and environmental characteristics. They argue that market controls in private schools are superior to political controls in public schools because the need to meet parents' concerns results in more effective school-level organization. Governance in public schools interferes with effective organization because it imposes restraints on school-level autonomy. They conclude that market controls produce more effective schools and advocate institutional reforms shifting schools away from direct democratic control and toward a decentralized system based on parental choice.

These conclusions are not justified by the argument and evidence that Chubb and Moe present. Their case for market control of schools goes wrong at three levels. First, they do not develop an adequate explanation for the different organizational characteristics of private and

public schools in the current system because they do not examine the influence of intervening factors on schools' organizational characteristics. They treat all schools' environmental and organizational characteristics, including the purposes of schooling, as endogenous and then fail to examine this assumption. Second, they infer directly from the current system of private and public schools to the institutional logics of market and political control without recognizing that their findings are contingent on the special character and advantages of private schools in the current system. Their empirical findings about private schools tell us little about market control of schools, given their failure to examine how supply and parental demand in a universal market system relates to supply and demand in the current private system. Third, and most fundamentally, they focus exclusively on the choice between politics and markets and develop biased and deterministic models of educational governance within those systems. They define away the complex and interactive processes of institutional development that result in substantial variance within the systems and, in some cases, mixed modes

of governance. These flaws, taken together, strongly undermine Chubb and Moe's conclusions about public and private schools and market and political systems of control in education.

Comparing Private Schools and Public Schools

Chubb and Moe's primary aim is to evaluate market and political institutions of control in education. At the empirical center of this evaluation, however, is a comparison of private and public schools' organizational characteristics: school-level autonomy, intraschool relations, and school goals and practices. They find that in private schools principals have greater authority and exercise more leadership, parents are more interested in their children's education and more involved with the school, the schools' goals are clearer and more focused, and teachers have greater influence and are more satisfied.¹ They point out that these findings reinforce other research reporting that private schools are more effective than public schools (Coleman and Hoffer 1987; Coleman, Hoffer, and Kilgore 1982).² Chubb and Moe recognize that many factors influence schools' organization, such as school size, the homogeneity of student bodies, and the self-selection of parents who send their children to private schools. They explicitly choose not to control for these factors in interpreting their survey findings. They argue that these factors provide the basis of appeal to parents and that it is the market that gives private schools the incentive to maintain them. In short, the organizations of schools and their relations with their environments are endogenous to the system of institutional control, and it would be inappropriate to control for those differences (pp. 1071-73).

This grand assumption follows from

their exclusive focus on the institutional choice between politics and markets in education. Given this focus, incorporating the features of the private and public school systems within their models is valid, at least as a working assumption.³ However, for reasons I explain in the third section, this focus defines away key theoretical and empirical questions about the development of institutional arrangements in education. The deterministic models they use simply assert without examination that all organizational characteristics are endogenous to the choice between markets and politics. Their decision not to examine the influence of other environmental factors on school organization also undermines using the results reported here for the more limited purpose of comparing the organizations of private and public schools directly. Their approach precludes the examination of alternative explanations and intervening forces in the organization of private and public schools.

In addition, they treat the *ends* of education as endogenous. They assert neutrality on the issue of purposes for education, explaining that choosing between parents' concerns and society's goals would require "a fundamental judgment about what the schools ought to be doing" (p. 1070). However, in choosing not to impose their own values for education, they are also rejecting the role of public deliberations concerning the purposes of education and denying the character of education as a public good. Some parents' concerns are outside the scope of public concern, such as children's religious training; and other parents' concerns are counter to public policy, such as desires for racial segregation or religious intolerance. Their point of view is troubling precisely because abdication of collective determination of purposes and the notion of education as a private good is inherent in the market control of schools they advocate (Gutmann 1987).

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Using Empirical Research To Address Institutional Questions

Chubb and Moe's conclusions address the institutions of market and democratic control, but their research findings are contingent on the current system of private and public education. Private schools have important advantages over public schools in the current system. Most children attend public schools and parents who choose private schools tend to be more active and interested in their children's education as well as better off financially (Coleman, Kilgore, and Hoffer 1982). In addition, private schools' student bodies are more homogeneous than those in public schools. These advantages influence the survey results, but they are not inherent in the market system of control. Even if we were to accept their conclusions about the superiority of private schools' organization in the current system, that tells us little about how schools would operate under a universal system of market control.

Reasoning from current observations to a more inclusive or universal market system in education requires taking into account both the supply of schools and parents' demand for them. Chubb and Moe's survey of schools does not provide an empirical basis for such an extrapolation of the institutional logic of markets.⁴ Supply and demand are closely related. Indeed, market theorists argue that supply would rise to meet parents' school demands. For that reason, and because we know more about parents' demands for education, I will focus on the question of demand. Prior to that, however, it is important to recognize that we know little about how the supply of education would work in a universal market system. In particular, we do not know how the existing public school system would evolve under market control. It seems unlikely that there would be sufficient interest on the part of school entrepreneurs and

parents to result in new schools with the organizational characteristics of current private schools that would serve all children (or that sufficient pressure would be put on public schools that they would act like private schools). Some children would not have access to these "better" schools and would be left in schools like those they currently attend or (more likely) schools depleted by the exit of interested parents and able children (Hirschman 1970).

In using current private schools to advocate market control for schools, Chubb and Moe must assume that all parents are like (or would become like) parents who currently send their children to private schools and therefore that parental demand in a universal market system would result in schools that look like current private schools. However, as they point out, the fact that parents in the current private system are more interested and active in their children's education is one key factor that makes private schools better. Projecting how the universe of parents would choose if given the opportunity (without cost) involves trying to answer several difficult questions: What kinds of factors would parents consider important? What proportion of parents would consider alternative schools? How many alternatives would be considered? How would parents obtain information about schools? Parental choice of schools has been adopted in only limited circumstances in the United States, so we do not know the answers to these critical questions. However, parents in Scotland have been given strong rights of choice within the public school system. I want to use the findings of research in Scotland concerning parents' school choices to raise questions about how such a system might work.⁵

In 1982, legislation essentially gave Scottish parents rights to send their children to any public school as long as the school had not reached its physical capac-

ity. Few parents in rural areas made school requests, as alternative schools were usually some distance away. Within towns and urban areas, substantial numbers of parents (between 10% and 25%) requested schools other than their district school. Even in these areas, most parents did not *consider* schools other than the district school. Of those who did consider an alternative school, most stayed with the district school anyway. And for parents requesting an alternative school, the most common reasons involved the nearness of the school and the attendance of the child's friends and siblings at the alternative school. Parents' requests have resulted in several popular schools being filled to capacity. Other schools have become seriously underenrolled, receiving less than a hundred new students a year, two-thirds the number considered the minimum for a viable school. These schools are expensive to run, they must offer restricted curricula, and they have lost many of their more able pupils, thus depriving the remaining pupils of important stimuli for learning. At the same time, parents of children still at those schools strongly resist attempts to close the schools.

What is more, there is little evidence that the more popular schools are more effective schools. Parents' concerns for the convenience of travel to the school act as a strong constraint on the operation of demand. Only a minority of parents choosing schools cited concerns about the schools' educational records or discipline at the schools or other reasons related to school effectiveness. Even here "success" was often primarily a matter of the kinds of pupils who resided in the school's district or the history of the school (Willms 1986). Schools cited for poor examination results and discipline problems were invariably located in economically and socially disadvantaged areas. Schools cited favorably were usually located in the more well-to-do areas and had been

selective schools before Scottish schools had been reorganized along comprehensive lines. (Indeed, some parents explicitly choose schools to avoid the "bad children" who attend their district school.) These surveys raise serious doubts about the validity of concluding that popular schools are "good" schools that should be rewarded or that unpopular schools are "bad" schools that should be penalized. Such a policy would likely result in increased disparities between schools in well-to-do areas and those in less-disadvantaged areas (Adler, Petch, and Tweedie 1989).

In sum, Chubb and Moe tell us something about the differences between market and political controls in current conditions; but they are not in a position to extend those conclusions to the operation of the market (or democratic controls) outside of that context. They do not give us any empirical basis on which to predict the crucial behavior of parents or school entrepreneurs as we move away from the current system of public and private education. Predicting their behavior requires reliance on other sources of data, and these suggest that market control of schools holds significantly less promise than Chubb and Moe claim.

Framing Institutional Questions

Chubb and Moe frame the question of institutional choice in education exclusively in terms of reliance on politics or markets. They also pack a great deal into their institutional logics of market and democratic control. According to Chubb and Moe, the choice between markets and politics determines all lesser decisions about education policy and school organization: school-level autonomy, principal-teacher-parent relations, and school goals. Their approach fundamentally mischaracterizes the nature of institutional choices in educational governance. It also

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excludes inquiry into the development of organizational structures and practices in education and the various influences of politics, markets, and other factors on those structures and practices.⁶

Chubb and Moe's analysis of private schools is driven by the idea of the market. Their emphasis on the market leads them to exclude discussion of other elements in the institutional structure of private education that would seem to be vital, such as the Catholic Church's doctrine and hierarchy and the professional ideology of the teaching profession. In their approach, market control results in principals' being given autonomy; and responding to parents' concerns, principals' decisions determine the structure and operation of the organization. Surely this misses much, if not most, of what is institutional about private schools. Their discussion of democratic control in public schools takes a broader range of considerations into account—bureaucracies, teacher's unions, and interest groups, for instance. However, even in this discussion, they attribute the entire institutional structure of public schools to the initial choice of democratic control. School-level autonomy and policy coherence are eliminated by the self-interested actions of the various parties involved in schooling—bureaucrats; teachers and their unions; and associations representing minorities, handicapped children, and textbook publishers, among others.

In addition to being deterministic, these models of market and democratic control are biased. In democratic control, participants' selfish motives always undermine educational quality. Bureaucrats, politicians, teachers' unions, and other interest groups all pursue their own agendas; and those agendas are seemingly never related to improving education. Parents are always frustrated in this system. Parents' concerns and the responsiveness of school officials to those concerns are portrayed more benevolently in the market model.

The market for education seems to work flawlessly, harnessing the self-interest of parents and school officials to produce efficiency and the common good. Chubb and Moe present no evidence to justify this strong characterization of politics and markets.

While we should aspire to address basic questions concerning the effects of market and politics, Chubb and Moe err in treating that choice as the sole causal factor rather than as part of a complicated and interactive process out of which come the varied institutional structures of private and public education. At the very least, the burden is on them to demonstrate that the choice between markets and politics explains so much of schools' organizational characteristics that it is justified to focus exclusively on those factors. They do not examine this question and instead report the differences between private and public schools as if they were confirmation of that point of view.

Institutions have recently become an important focus of inquiry in political science (Knott and Miller 1987; March and Olsen 1984; Smith 1988). However, compared to Chubb and Moe, others addressing the "new institutionalism" adopt a different, more detailed focus. For instance, March and Olsen stress the importance of political structure—the "collection of institutions, rules of behavior, norms, roles, physical arrangements, buildings, and archives that are relatively invariant in the face of turnover of individuals and relatively resilient to the idiosyncratic preferences and expectations of individuals" (1984, 741). Smith (1988) also emphasizes the importance of cognitive structures, such as popular belief systems and professional ideologies. These authors emphasize the importance of examining the origins and development of these institutional structures, recognizing the importance of strategic action, history, chance, and the influence of other institutions.

Describing the governing institutions in education, their origins, and how they affect the operation of schools involves considerably greater richness and complexity than Chubb and Moe's approach allows. For instance, the relative autonomy of school principals is an important element of an institutional understanding of schools. Principals' autonomy is not determined by whether the school is private or public but varies considerably within those types of schools. (This alone would seem to challenge Chubb and Moe's simple focus on markets and politics.) In part it is determined by structural factors such as state education law, union bargaining, church doctrine, and teachers' professional norms and by policy decisions about such issues as school size and curriculum. To some extent, it is also a matter of choice. Educational policy makers argue for greater school-level autonomy in public schools (Finn 1984). And despite Chubb and Moe's pessimism about the prospect of political reforms (p. 1085), the logic of democratic control does not preclude the possibility of deliberate institutional change.⁷ In looking at the governing institutions of education, we should be focusing our investigations on these and other factors that influence principals' autonomy, not only whether schools are controlled by market or political institutions.⁸ Indeed, all of the organizational structure and patterns of practice that Chubb and Moe look at in private and public education fit within the scope of this institutional focus. Their origins as well as their effects should be the focus of detailed study that looks at a much broader set of factors than questions of market and political systems of control.

Conclusion

Chubb and Moe have raised fundamental questions about the roles of markets

and political institutions in education. These questions are particularly important because of the ongoing debates about parental choice in education and the growing emphasis on market institutions in the study of public policy. Chubb and Moe's study of organizational structures and patterns of practice in private and public schools provides important new data with which to address these questions. However, they are mistaken in looking at these results and educational governance solely in terms of their models of politics and markets. Instead, we should bring Chubb and Moe's perspective—especially their empirical study of schools' organizations—into the more detailed and focused studies of organizational structures and patterns of practice in education and their effects on the operation of schools and the education received by students.

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John Chubb and Terry Moe tell us that private schools are more effective than public schools. More to the point, they tell us that private schools are more effective precisely because they are private, that is, because they are "largely autonomous actors 'controlled' by the market" (p. 1065). The argument, however, ultimately fails. Private schools may be more effective. They may indeed be more effective because they are private. But market controls have very little, if anything, to do with it.

Chubb and Moe give us a clear and concise summary of their basic argument: "In the private marketplace, educational choice is founded on what has come to be called, following Hirschman (1970), the *exit* option. If parents and students do not like the services they are being provided, they can exit and find another school

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whose offerings are more congruent with their needs. This process of selection promotes a match between what educational consumers want and what their schools supply" (p. 1068).

Following this line of reasoning, for market controls to produce private schools that provide more effective education two things would have to be true. First, more effective education would have to be the most important ingredient in the parent-student decision to attend a particular private school. Second, the *exit* option would have to be easy to exercise, that is, not subject to very many or very powerful constraints. I seriously doubt that either of these is true in the real world of private secondary education.

Choosing a Private High School

Start with the motivations to attend a particular private high school. Without question, parents—and even prospective students—will cite a reputation for educational effectiveness when explaining why they chose a particular school. In fact, it is likely that a reputation for excellence really does play a role in school selections. But in most such choices I am convinced it plays a consistent and only moderately important second fiddle to something bigger—either religion, race, or class.

Religion is the most important "first fiddle," of course. First of all, nearly 60% of the private secondary schools in the country are Catholic schools, and it is no accident that most of the students in these Catholic schools are from Catholic families and that most of the Catholics who attend private high schools—other than the fortunate few who attend the elite private schools—attend schools that are Catholic. There are all sorts of reasons for Catholic parents to send their children to Catholic high schools, not the least of which may be the fact that the parents themselves at-

tended one, maybe even the one they plan to send their kids to. Size, atmosphere, academics, a chance to compete in all sorts of extracurricular activities may all enter the selection decision. But for Catholics, the question is almost never, "Should we send our child to a private school?" but "Should we send our child to a Catholic school?" Then it is time to talk about academics.

Second, Catholics don't have a monopoly on religiously affiliated secondary schools. Other religious denominations operate over 10 thousand schools. (Statistical Abstract 1988, p. 136). If even 2% of those schools are high schools, three out of every four private high schools in the country is in the sense of self-identification a religious school. Mainstream Protestants may not be very concerned about the religious affiliation of the private schools they consider for their children, but evangelicals and other fundamentalists most certainly are. So are Mormons, Seventh Day Adventists, and many, many others. Once again, academic concerns play a secondary role in the choice.

Painful as it may be to admit, more than a handful of U.S. teenagers end up in private high schools as a means of dodging school integration. Not all that many parents are likely to talk very openly about it, but of the just over a thousand non-Catholic secondary schools in operation in 1986, nearly two-thirds were founded after James Meredith first enrolled at the University of Mississippi; while of the nearly 5 thousand schools that combine elementary and secondary instruction in a single school, just under 80% were founded in the past 25 years (Statistical Abstract, 1988, p. 136). Not all of this school building activity was motivated by a desire to perpetuate school segregation, of course. Some of it may have been religiously inspired, since 45% of the over 10 thousand non-Catholic religious schools operating in 1986 opened in 1976 or later (Statistical

Abstract, 1988, p. 136). Still, since so many of these new schools—including those built by people more than willing to state the school's religious affiliation—were built in the South, it is hard to escape the conclusion that for some students the choice to attend a private secondary school is a choice to avoid going to high school with blacks. Where does that leave academics?

Finally, there is the matter of class. It is not a matter of relevance to a very high percentage of future high school students, of course; but it is likely to be of great relevance to that group headed for the elite private high schools. Academics may play a larger role in such choices, but few people seriously question the academic rigor of the elites; and the choice is almost always among the elites, not between one of those schools and some other type of private high school, let alone between one of the elites and the local public high school. The factors that will enter such a decision will be many and varied. They may include following—or avoiding—the family footsteps, the placement record a particular secondary school has compiled at an already-chosen college, even distance from home; but in some fundamental sense the decision is to attend a particular type of secondary school, the type of school “our people” have always attended, and academic excellence is taken for granted when the choice is made.

I am not suggesting that private high schools don't have to worry at all about their academic reputations. Nor am I suggesting that academic considerations play no role in the student-parent choice to attend a private high school instead of a public one or in the choice of one private high school over another. But I am saying that for most students and parents, the primary factors in making those decisions are not academic. They are religion-, race-, or even class-oriented. If I am right, market forces generated by a school's customers are not likely to lead it toward academic excellence.

Exits May Not Be So Easy To Find

But market forces aren't likely to lead contemporary private high schools much of anywhere. At least, they are not likely to if the main mechanism for enforcing them is, as Chubb and Moe contend, the exit option; for that option is far harder to exercise than they would have us believe.

First of all, the most-often-thought-of exit—to the public schools—may be totally unacceptable both to the student and the parents. To begin with, if the prime movers of students and parents are religion, race, and class, the public schools almost certainly cannot provide those things. In a good many small towns, they can still provide racial homogeneity; but by definition they cannot provide religion, and even in the most affluent of suburbs the local public high school is not Groton, Choate, or Andover. Beyond that, in many cases the decision to enroll in a private high school is at its heart a decision to avoid the local public high school. Private high school students may well be going away from something just as much as they are going toward something. If they are, they aren't going to want to go back. Even if they find that the private high school they have selected isn't exactly what they hoped it would be, at least it isn't the public high school. As long as they feel that way, the easiest exit option isn't much of an option at all.

Second, the decision to leave one high school for another is rarely an easy one to make and involves things far removed from academics. Such a decision has all sorts of implications for peer relationships—romantic and otherwise. It also affects opportunities for extracurricular participation. A smaller high school has fewer talented folks to choose from, whatever the activity might be. Then there is the teenager's fear of the unknown. (Parents aren't too terribly confident about it either). There may be problems at their high school, but at least they know what they are.

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One measure of our assessment of the psychological toll extracted by changing high schools can be found in the number of young people each year who live extended periods of time with a friend or relative so they can "finish high school where they started." Spouses have been known to live apart for extended periods to allow a child to finish at least a year, sometimes even an entire high school career. My father did a hundred-mile round-trip to work each day so that my younger sister could spend her senior year in the high school we had both attended. People who will go to those lengths to avoid asking a teenager to change high schools, will not be likely to turn around and make such a request even if they do find themselves becoming dissatisfied with the academic climate at the school they have recently chosen.

Finally, the exit option requires some place to go. A student cannot opt out of high school. He or she has to change high schools. If, as I have argued, the local public high school will not be acceptable, there has to be another private high school available.

Half of all U.S. citizens still live in cities with fewer than 100 thousand people, and nearly one-third of us live in cities of under 50 thousand population. If you live in a city of 40 thousand people and want to send your child to a Catholic high school, you are almost certainly going to have only one choice. If there is another nearby city with such a school, there is the possibility of commuting; but that will bump up against the all-my-friends-are-going-here-you-know argument (few parents know a surefire response to that one), and in any event there is a limit to the amount of time, money, energy, and worry parents want to devote to the process of getting a child to and from high school. Even cities of 100 thousand are unlikely to have more than two Catholic high schools; and if a parent is looking for an evangelical Christian academy, the

pickings get even slimmer. The same sorts of problems occur for parents who want to make sure that their child does not attend a high school "dominated" by blacks. There just may not be very many options out there. In fact, in all but fairly large cities the parent who wants a private high school for educational excellence still may find a very small range of options if the initial choice does not pan out.

Exit options are clearly more available for people living in major metropolitan areas, but even in these areas there are serious constraints. Getting from home to school is almost certainly going to be the biggest one, particularly if people are not confident of the safety of public transportation; but we have also reached a point at which most large metropolitan areas are something very much like psychologically balkanized. People from the area's northern suburbs rarely see its southern suburbs, and people from the westernmost reaches of the area are quite thoroughly unfamiliar with its eastern recesses. It just would not occur to a northern suburb family to look at southern suburb high schools, or vice versa.

Conclusion

Like most people arguing the virtues of markets, Chubb and Moe tend to paint a world of rational individuals pursuing clearly understood and easily articulated goals with only limited constraints on their ability to act in a manner that will advance those goals. There is no question that parents willing to foot the bill for private secondary education really do want their children to get a quality education. Nor is there any question that they will try to choose a school that will provide one or that they will complain if they don't think one is forthcoming.

But parents about to pay for a private high school education want other things besides a quality education. Indeed, those

other things may well take precedence over educational quality when school selection decisions are made. Beyond that, once a school has been chosen, the traditional market option of getting out if dissatisfied is a decision constrained by a number of factors, most of them quite unrelated to the quality of the education the school is dispensing. Fear of the local public high school, a starting spot on the varsity basketball team, the lead in the spring play, or a two-hour commute to an academically superior private school can easily dissuade all but the most dissatisfied of a private high school's customers.

Two questions remain: Are private high schools more effective than public high schools? If so, why?

As to the first question, I am more than content to accept Chubb and Moe's assertion that the work of James Coleman, Thomas Hoffer, and Sally Kilgore (1982) and the subsequent work of Coleman and Hoffer (1987) demonstrate the greater educational effectiveness of private high schools. Chubb and Moe's own findings that private high schools exhibit the characteristics normally associated with educational excellence certainly provide strong reinforcement for the Coleman, Hoffer, and Kilgore position.

As to the second question, I see two factors contributing to the increased educational effectiveness of private high schools. First, as Chubb and Moe themselves point out, private secondary schools have an incentive to create effective voice mechanisms to allow parents and students to have a real say in how the schools are run (p. 1068). It is just possible—on this small scale, with a far more homogeneous set of people trying to use those voice mechanisms and with no real conflict generated by powerful unions or even more powerful professional bureaucracies—that voice mechanisms can work.

Second (and to my mind even more important), it is quite possible that the edu-

cational superiority of private high schools is the ironic triumph of professionalism in education. Clearly, it is not the triumph of the phony professionalism so often trumpeted by teachers unions or educational bureaucracies. But it just might be the triumph of the real professionalism that so many teachers initially bring to their work. Freed from all of the forces that Chubb and Moe so clearly show work in opposition to educational excellence in the public sector, teachers and principals can work to educate students. They don't provide effective education in response to pressure from parents who are ready to move their children to another school at the drop of an adverb. They provide it because that is what they want to do, and their environment allows them to do it.

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Markets Are Imperfect: Facts

A standard line against allowing market forces to play a greater role in educational decision making is that educational markets are imperfect and will not have the advantages that economists and others claim for them. This is the essence of Riley's critique.

Riley focuses on two weaknesses that allegedly prevent markets from working. First, parents are less motivated by academics than by religion, race, and class in choosing to send their children to private schools. Second, the exit option is quite limited in practice: public schools are not feasible alternatives since they cannot cater to religious, racial, or class concerns; there are few private schools to choose from; transportation costs further restrict the feasible choices; peer relationships and fear of the unknown cause children to resist changing schools.

Factually, his argument is based almost

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entirely on speculation. Private schools and the parents of children who attend them are stereotyped and mischaracterized. It is true that most private schools are affiliated with a religion. It is true that a small number of elite schools cater to the well-to-do. And it is true that in the early days of busing, certain private schools (especially in the South) were havens for whites who feared integration. But it is wrong to jump to the conclusion that religion, race, and class are what private schools are all about (Kraushaar 1972).

To be fair to Riley, good data on the universe of private schools are hard to come by. But most students in the private sector attend Catholic schools, and there is plenty of data on these schools to belie the stereotypes. Nationwide, some 23% of the students in Catholic schools are minorities (Brigham 1989, p. 17); and the percentage is often significantly greater in urban areas. Black enrollment alone exceeds 15% in the Catholic elementary schools of Baltimore, Chicago, Detroit, Newark, New Orleans, New York, and Washington (National Catholic Education Association 1989). In these and many other large cities, the public schools are often so bad that blacks (who are seldom Catholic) and Hispanics are coming to see the Catholic schools as a crucial avenue of escape from a system that has so utterly failed them. This account of Chicago's schools is indicative of recent developments:

In some 140 of Chicago's 242 Catholic schools, black and Hispanic students make up more than 80 percent of the enrollment. In most of these schools, Protestants outnumber Catholics. . . .

The Chicago public school system, by virtually all accounts, has fallen into serious disarray, beset by violence, teacher shortages, and inadequate budgets. Last year, average student scores at more than half the city's schools were in the lowest 1 percent nationally. . . .

By contrast, the Archdiocese says that 85 percent of its students go on to college; among inner-city youths attending Catholic schools, the figure is 66 percent. (Dirk Johnson, "Catholic

Schools Reach Out To Serve Poor and To Borrow," *New York Times*, 13 September 1988)

Generally speaking, the commonly held notion that private school students are basically rich and white is grossly inaccurate. The fact is that on socioeconomic as well as racial grounds, the students attending Catholic schools are a virtual cross-section of U.S. students nationwide (Coleman, Hoffer, and Kilgore 1982; National Catholic Education Association 1985).

Also inaccurate is the notion that parents do not place heavy weight on academics in choosing to go private. There is lots of evidence that private school parents are more concerned about education, monitor their kids' performance more closely, have higher expectations, and participate more actively in school affairs than parents of public schools students do (Chubb and Moe 1990); Coleman, Hoffer, and Kilgore 1982). In a recent national survey of Catholic schools, moreover, principals, asked for their perceptions of what motivates parents, responded that parents were motivated by academic concerns *above all else* (National Catholic Education Association 1985). If parents are in fact primarily motivated by religion, race, and class, the schools are not aware of it.

Finally, it is wrong to suggest that the educational marketplace can offer parents few real options. To begin with, public schools are surely feasible alternatives for most private school parents. They are ruled out only if one accepts the mistaken notion that what parents really care about is religion, race, and class. Secondly, there are lots of private schools in this country, some 28 thousand of them. Since the 1930s, their number has increased by about 120%, roughly in proportion to private school enrollment. Over the same period, in the face of a 60% growth in public school enrollment, the number of public schools (excluding one-room

school houses) has actually declined by 26%—with a corresponding increase in average school size (National Center for Education Statistics 1988, 10, 83).

The availability of private schools is all the more impressive given that the deck is stacked against them since they must compete with “free” public schools that even private school parents are forced to support with their tax dollars. Imagine how many schools there would be—and how many alternatives parents would have—if this bias were eliminated. To put it more positively, imagine how many schools there would be under a different institutional system that, rather than strictly controlling the numbers of schools, allowed new schools to emerge in response to parental demand. As things now stand, the most important limitations on exit alternatives are due to government controls, not to anything inherent in the market.

Markets Are Imperfect: Theory

We do not argue that markets are perfect. No one does. It is easy enough to see that parents are sometimes motivated by more than just academic excellence in their choice of schools, that they don't have zillions of schools to choose from, that transportation is costly, that education involves various spillover effects not internalized in parental choice, and, in general, that there are many imperfections that may inhibit the efficient operation of market forces.

It is legitimate to point out these sorts of things. But little is accomplished in stopping there (as Riley essentially does), as though a listing of imperfections somehow seals the case against markets. It clearly does not. In fact, it is inappropriate to think that any kind of persuasive case can be made, whether for or against markets, until a pivotal question is addressed: how do markets, imperfect

though they may be, compare to the United States' traditional institutions of direct democratic control as structural means of bringing about the kinds of schools that citizens want for their children and for the nation as a whole? It is precisely this comparative institutional question that Riley and most critics of markets never get around to addressing.

Our own theory is explicitly comparative. It is about markets, but it also about politics. Indeed, largely because the logic of markets is so much better understood, we spend most of our time arguing that there is an analogous logic that governs how the traditional democratic institutions within public education—school boards, superintendents, central office bureaucracies, state legislatures—function as a system to shape the organization and performance of schools. We show that these institutions tend to nurture school organizations that are ill suited for effective academic performance. This obtains even if everyone involved genuinely wants good schools—which is pretty much the case. The problem is not the people but the institutions. Bureaucratic control, driven from above by the pressures of democratic politics, functions naturally and with the best of intentions to provide people with just the kinds of schools they don't want.

We “advocate” markets, then, not because they are somehow wonderful in the abstract but because, in getting away from direct democratic control and substituting a form of indirect, bottom-up control, they provide a context more conducive to the emergence and survival of effective school organizations. Markets are not perfect, they are just better. Critics will have to offer a comparative analysis of their own, showing that current institutions are in fact better designed than markets to promote effective organization. That's a tougher job than simply complaining about how imperfect markets are.

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Our Theory Is Too Simple

Tweedie develops three basic lines of criticism, the most general of which is that our institutional theory is too simple. In his view, we are trying to explain the organization of schools purely by reference to their surrounding institutions when in fact things are far more complex than this. An adequate theory would take systematic account of a catalog of other relevant factors: church doctrine and hierarchy, teacher professionalism, state laws and policies, unions and collective bargaining agreements, and so on.

This is largely a matter of methodological orientation. Tweedie subscribes to the "many-factors" school of methodology, namely, that social phenomena are highly complex, involving lots and lots of variables, and that the pursuit of comprehensive explanation inevitably sets for social scientists the task of identifying and taking into account as many of these variables as possible. Good theories have lots of variables.

This is not our orientation. Good theories are simple. They do not try to explain everything (which is impossible anyway) but are designed instead to cut through most of the complexities and capture only the most essential features of the thing being explained. The institutional theory we develop is designed to provide insight into how schools are pervasively shaped by their surrounding institutions and into the constraints that institutions—often indirectly, without conscious choice—place on the kinds of schools that can prosper within them.

In the end, our theory does not actually leave out everything that Tweedie and others consider relevant. Unions, teacher professionalism, and all sorts of "variables" are in fact integral parts of the theoretical story we have to tell; but the roles they play and the reasons for their importance flow from a simple, core perspective on institutions. These variables

are relevant, but are not fundamental. Institutions are fundamental. And it is by understanding the nature and logic of institutions that we understand why and how these other variables are relevant in the first place. Our theory is simple, therefore, not because it takes a stand on what is fundamental and because this in turn imposes coherence and order on much of the rest, which otherwise appears overwhelmingly complex.

The problem with the many-factors approach so common in social science research (and certainly in education research) is that it gives in to complexity. It seeks comprehensive explanation by amassing relevant variables, but its labyrinthine theories tend to do little more than reflect the complexity they are supposed to be explaining. They have a lot to say about the trees and almost nothing to say about the forest. We would rather try to talk about the forest even if we miss some of the trees.

We Don't Control for Relevant Variables

Our statistical analysis is essentially a descriptive exercise. We use simple dummy variable regressions to show that public and private schools are systematically different along important dimensions of organization and environment. These differences are precisely what our theory would lead us to expect. Tweedie takes us to task, however, because our regressions contain no controls for the many factors other than sector that are presumably relevant to an explanation.

Generally speaking, of course, good statistical work requires controls for all relevant variables. But what is relevant, exactly how is it relevant, and exactly how should it be controlled? These are crucial issues that all too often are met with canned responses. Laundry lists of variables thought to be important are

automatically appended to linear regressions as "control variables." This has the appearance of methodological rigor. In fact, it threatens to be a mindless exercise that could easily do more harm than good.

For reasons discussed in our article, an institutional perspective on schools suggests that the major variables of interest are endogenous and integrally bound up with one another. This means that modeling their interconnections is no simple matter and that routinely tossing "control variables" into single-equation regression models as separate, additive, exogenous influences misspecifies the role we claim for them. This is especially problematic when sector itself is treated as a variable, as it is in our public-private comparisons, because institutions are the driving force behind the configuration of virtually all the other variables, including those our critics would presumably want us to use as controls.

Consider school size, which is surely relevant when it comes to explaining many aspects of school organization. Should we control for size in investigating whether, say, teacher collegiality varies with institutional context? Our answer is *no*. The theory suggests that size is an integral part of the distinctive organizational syndromes associated with each of the two sectors. Schools tend to be much smaller in the private sector for institutional reasons related to how politics and markets work; and this has all sorts of consequences for other aspects of school organization. These effects of size, however, are not separate from the effects of sector. Indeed, they are part and parcel of what we *mean* by the effects of sector. So what is the point of designing a single-equation regression model to estimate the "partial effects" of sector by controlling for school size? What appears on the surface to be methodologically rigorous—controlling for a relevant factor—makes no sense under the circumstances.

The conclusion to be drawn is not that we think controls are unnecessary but that whether and how they are used depend on the theoretical purposes of the analysis. "Politics, Markets, and the Organization of Schools" is a simple attempt to present basic—and heretofore unavailable—evidence on the organization of public and private schools in a way that sheds light on our general theoretical notions about how the two institutional contexts should differ. Given this theoretical purpose, a dummy variable regression without any controls whatever seems to us both enlightening and appropriate.

This article grew out of a larger project. In the early stages of the project, we viewed a simple public-private comparison as a first step toward a much more comprehensive analysis in which we would eventually model a simplified version of the full causal structure. We would no longer use sectors as proxies for complex combinations of variables but would actually try to specify how the major variables are related to one another, presumably by building systems of simultaneous equations. We had visions of gigantic LISREL models.

After much effort and frustration, we eventually realized that this was an impossible dream, at least until much better data were available. We chose a compromise path that we think is reasonable and that Tweedie and other critics may find more to their methodological liking. This line of empirical analysis—which seeks to document the links among environment, organization, and performance for schools generally, regardless of sector—is developed in our forthcoming book, *Politics, Markets, and America's Schools*. Here we take various steps to try to minimize the endogeneity problem, then proceed to build and test linear regression models that do incorporate controls for the kinds of variables that most concern our critics (e.g., student aptitude, social

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class, selection bias in the choice of schools).

It is dirty pool, we realize, to respond to Tweedie by relying on analysis he has not even seen. But the fact is that the argument and findings of our article are uniformly supported by the far more extensive analysis of the book. What we show—to put it most simply—is that certain organizational characteristics are associated with effective academic performance, that organizations having these characteristics are most likely to emerge when schools operate with substantial autonomy, and that institutional contexts that function to promote autonomy (as markets do) are more conducive to effective organization than those that function to constrain autonomy (as current political institutions do).

It Is Misleading To Compare Existing Private and Public Schools

Tweedie warns that a comparison of public and private schools as they now exist is a poor basis for “extrapolating” to what a universal market-based system would look like in practice. Existing private schools are a privileged bunch. They skim off the best students, parents are more interested and involved in school affairs, families are wealthier, schools are more homogenous, and so on. This makes markets look better than they are. If all students and parents were placed within a market-based system, schools would no longer have any privileges, and their organization and performance would suffer accordingly. They would not look like the private schools that are out there now.

Tweedie’s line of argument confuses what ought to be understood as two separate claims. The first is that our institutional theory is inductively derived from misleading evidence about public and private schools. The second is that evidence about sectoral differences is a misleading

basis for testing an institutional theory. Whatever he really means to say, we disagree with both.

Regarding the first claim, it is wrong to suggest that we are simply extrapolating from observed public-private differences in arguing the advantages of markets. This is not an exercise in grass roots empiricism. What we have to say is anchored in a theoretical perspective on the nature and logic of institutions, with attention to the distinctive ways in which they structure the incentives, resources, and opportunities of the various actors within them. Ours is an attempt to explain why schools look the way they do as well as to explain how they would look under different institutional conditions—not an attempt to describe and extrapolate.

Regarding the second claim, Tweedie believes that the systematic differences we uncover are due to privilege, not to institutions, and that we are unjustified in claiming that the evidence supports our theory. This is a legitimate worry, one that we have kept in mind throughout our project. Under existing institutional arrangements, private schools are indeed privileged along roughly the lines Tweedie sets out (although, like Riley and many others, he tends to exaggerate the extent of it). But this does not explain away our empirical results.

As Coleman, Hoffer, and Kilgore showed in *High School Achievement* (1982), and as follow-up work has continued to demonstrate (Coleman and Hoffer 1987), private schools outperform public schools even when controls are introduced for student aptitude, social class, and other factors that presumably give private schools privileged status. A veritable mountain of research in the “effective schools” literature has associated effective academic performance with certain organizational characteristics on the part of schools (Purkey and Smith 1983). And our own research shows (just as one would expect if both Coleman, Hoffer,

and Kilgore 1982 and the "effective schools" research are basically correct) that private schools possess these desirable organizational characteristics to a far greater extent than public schools do.

Research reported in our book, moreover, shows that these kinds of organizations flourish in institutional environments that grant schools substantial autonomy, as markets do. We also show that the crucial link between autonomy and effective organization holds even when attention is limited to the public sector alone: public schools whose governing institutions grant them greater autonomy tend to have more effective organizations than public schools whose governing institutions are highly constraining. All this is demonstrated in the presence of controls for privilege factors like social class and student aptitude. On the whole, then, while it makes sense to suspect that public-private differences are really just due to privilege, the weight of the evidence strongly suggests otherwise.

Finally, we want to take issue with Tweedie's claim that a comparison of sectors as they currently exist tends to exaggerate the apparent advantages of markets. We think its net effect runs in the opposite direction. Opponents of markets use public-private comparisons all the time to document quite unwarranted claims about how poorly markets would work if universally employed.

A standard argument against markets, for instance, is that parents are too poorly informed and too uninterested in their children's schools to allow markets to work well. The evidence: lots of parents and children in the public schools are poorly informed and uninterested. The inference: they would continue to be this way under a market system. The inference is simply unwarranted, for reasons that economists and social psychologists could both readily supply. In a system where virtually all the important choices are the responsibility of others, parents

have little incentive to be informed or involved. In a market-based system, much of the responsibility would be shifted to parents (their choices would actually have important consequences for their children's education), and their incentives to become informed and involved would be dramatically different.

Examples occur in all areas of social life and are easy to come by. But in education, perhaps the most graphic illustration comes from the schools in East Harlem, New York, where radical choice-based reforms have led to spectacular increases in parental interest and involvement among the urban poor, the people supposedly most threatened by markets and most in need of having a benevolent authority tell them what to do (Domanico 1989; Fliegel 1989).

Tweedie's own argument about the dangers of extrapolating from current arrangements is, ironically enough, based on an extrapolation from current arrangements. He relies heavily on research he and others have carried out in Scotland, where parents were given a measure of choice. The problem with his example is that the system was left basically intact. Parents could choose; but they had little to choose from, for there was no market-based change on the supply side to ensure that their demand would be reflected in a larger or more diverse—or simply different—pool of schools to select from. The schools were the same as before. This is nothing like a market-based system, and extrapolations based on Tweedie's data are simply unwarranted.

The situation in East Harlem is a much better approximation to what a market-based system might look like. There, not only were parents allowed to choose, but teachers were encouraged to hive off, form schools of their own, and compete for students with other such schools—which they did, with enthusiasm. The number of schools more than doubled. The result was a cornucopia of desirable

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alternatives for parents, one that radically changed their incentives and literally transformed their involvement in education.

Conclusion

If there is a single theme that best captures our position, it is the value of a unifying theoretical perspective that can lend coherence to the endless list of "relevant variables" that supposedly populate the educational world. Education research invests too much effort in detailed examination of all the myriad bits and pieces and far too little in developing theories that might help make sense of it all. The literature is buried in variables—all of them relevant but without any clear, coherent connection to one another.

The problem is nicely illustrated in Riley's concluding remarks, where he singles out two "factors" that he considers especially important in explaining why private schools outperform public schools. The first is that private schools have more effective voice mechanisms, the second is that they give fuller rein to teacher professionalism. We agree that these factors are important. The difference is that he treats them as disconnected, as two nice properties private schools happen to have that public schools don't; whereas the whole thrust of our argument is that their explanatory foundations are the same. Political and market institutions each place their own distinctive, identifiable stamps on the forms in which voice mechanisms, teacher professionalism, and a great many other properties of school organization emerge and take shape within the two sectors. Riley isolates them as important variables. We see them as integral parts of a larger whole.

The key to gaining a coherent perspective on the whole is theory. Our critics think that our institutional theory gives

too much credit to the much-despised market. It is far too simple. It leads us to justify carrying out statistical analysis without the usual laundry list of controls. It seems to benefit from an unfair comparison of existing public and private schools. As we said, we think these sorts of objections are plausible enough (at least on the surface) but that they are ultimately unconvincing. We want to add, however, that we also think our critics ought to do more than simply raise objections to our theory. They should be able to point to a theory that is better. Again, their proper task is comparative; and this is precisely the task they have avoided.

Is there an alternative theory, preferable to ours, that can pull together the many factors and offer a coherent explanation of schools? Tweedie points approvingly to the "new institutionalism"; but it is a misnomer to call this grab bag of diverse, often conflicting approaches a theory and quite wrong to think that it offers anything like a coherent perspective on schools. We have yet to be confronted with a well-developed competing theory. All we hear are objections. This is beneficial on the whole, but in the end our critics have to broaden their scope a bit and think more constructively about their mission. You can't beat something with nothing.

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Notes

1. Their survey findings generally support these conclusions, though there are some exceptions, particularly where findings for Catholic schools, elite private schools, or other private schools are not statistically significant for particular measures. I will not focus on these inconsistent results, although the inconsistencies weaken the argument that it is

market control that produces the differences between public and private schools.

2. Chubb and Moe note several articles that challenge these studies' conclusions and then simply assert that they have, in their judgment, withstood the challenges (1988, 1086).

3. They acknowledge that this assumption should be examined further, but they do not carry out this examination. Looking at the influence of school size, student body homogeneity, region, and rural-suburban-urban location would be particularly helpful in the interpretation of their results.

4. Some relevant evidence does exist, but Chubb and Moe do not use it. Several studies have been conducted of voucher experiments and choice within public school systems. These plans have been limited in scope (often focused on achieving voluntary desegregation), and studies of them have found mixed results; but they raise some doubts about the advantages of voucher plans and the workings of market systems in education (Cohen and Farrar 1977; Murnane 1986; Nault and Uchitelle 1982; Rossell and Glenn 1988).

5. The following two paragraphs state the conclusions of a study of parental choice in Scotland conducted by Adler, Petch, and Tweedie (1989). See also University of Glasgow Department of Education 1986 and Tweedie 1989. This research can be used only to examine the operation of parental demand as local authorities maintained control over the supply of education in Scotland.

6. By framing the institutional choice as one of politics or markets, they exclude consideration of mixed systems, such as allowing parents to choose within the public school system and alternative ways of structuring democratic or market control of schools.

7. Their pessimism is contradicted by the adoption of strong rights of parental choice in Scotland and more limited schemes of parental choice in many U.S. jurisdictions, including Minnesota and Cambridge, Massachusetts. For a survey of the wide variety of schemes that exist, see Raywid 1985. In addition, there is growing support for similar plans in many other locations, as well as a myriad of institutional arrangements in local education that are not consistent with their depiction of democratic control.

8. A second key issue involves examining how principals use their autonomy. Chubb and Moe state that given the discipline of the market, principals hire effective teachers. They emphasize external restrictions on principals' influence in public school hiring and how this interferes with leadership, the selection of effective teachers, and incentives for good teaching (pp. 1082-84). They ignore possible beneficial effects of job security and protection from political and personal favoritism and bias.

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CRIME AND PUNISHMENT: ARE ONE-SHOT, TWO-PERSON GAMES ENOUGH?

George Tsebelis argued in the March 1989 issue of this Review that decision theory is completely appropriate for analyzing games against nature but not appropriate for dissecting games against a rational opponent. Analysts who mistake a rational opponent for nature in constructing models commit what Tsebelis calls "the Robinson Crusoe fallacy." In this controversy, William Bianco and Peter Ordeshook attack components of Tsebelis's argument. Bianco believes the model should be set up as an iterated, rather than a one-shot, game. Ordeshook feels that proper modeling cannot rely merely on two-person games and, in addition, he argues that Tsebelis commits some technical errors. In his reply, Tsebelis joins the issues and buttresses his original analysis.

George Tsebelis (1989) shows that many analyses of crime and criminal behavior assume each actor chooses his strategy given fixed strategy choices for the police.¹ As Tsebelis points out, it is more accurate to assume the players make simultaneous strategy choices. He then presents a one-shot game in which two players—the police and the public—choose their strategies simultaneously. In this game, changes in the severity of punishments have no effect on the incidence of crime but do cause police to become more lax in their enforcement of law. Similarly, increased rewards for not violating the law (welfare measures) reduce the amount of law enforcement but not the crime rate.

I focus on a critical assumption in Tsebelis' analysis. While he discusses the relationship between police and public in terms of repeated interactions, his results and empirical implications are derived from a single-shot game.² Tsebelis' model is therefore a simplification of the phenomena he is modeling. A citizen's decision to speed is not made only once; rather, it is a series of decisions made over

time, whenever the citizen gets into a car. Similarly, a police officer must decide each day whether to enforce the law or not. Repeated interactions are also characteristic of the other relationships cited by Tsebelis: government regulators and regulated industries, leaders and subordinates in a hierarchy, and congressmen involved in an oversight relationship with an agency.

Results such as the Folk Theorem (Fudenberg and Maskin 1986; for similar results see Axelrod 1984) suggest it may be inappropriate to model iterated situations as one-shot games.³ In an iterated game, players can coordinate their strategies to produce mutually beneficial outcomes that are not equilibria in a one-shot version of the game. These agreements are sustained by the use of "trigger strategies" (Friedman 1971) whereby a player abides by the agreement as long as all other players do so. This behavior can be seen as the abstract equivalent of phenomena such as "trust," "norms," or "reputation." However, such phenomena cannot occur when players interact only once. Given the effect of iteration and the

fact that real-world interactions between the police and the public are repeated, it seems essential to generalize Tsebelis' one-shot results to the iterated case.

The changes produced by iterating the police-public game are striking. In the one-shot game, the only equilibrium outcome is one where players use mixed strategies. However, in some iterated versions of the game, pure strategy outcomes can be sustained as subgame perfect equilibria (Selten 1975) if players do not discount future payoffs "too much" (to be made more precise later). In an iterated game analyzed here, the outcome where the public does not break the law and police do not enforce the law can be produced as an equilibrium if players use appropriate strategies. In this game, welfare measures make it easier to sustain this no-crime outcome, rather than having no effect, as in the one-shot game (Tsebelis 1989, Theorem 4).

In sum, I agree with Tsebelis that decision theory should not be used to model game-theoretic situations. However I do not share his view that a one-shot game provides insight into the forces driving real-world crime and law enforcement.

Table 1 gives the one-shot, two-player, police-public game presented in Tsebelis 1989. In this game, the public chooses to speed or not to speed; the police choose to enforce the law or not (i.e., enforce or shirk). By construction, the public prefers to speed if the police shirk but not to speed if the police enforce the law. The police prefer to enforce if the public speeds but

to shirk if the public does not speed. These assumptions are specified in terms of restrictions on the player's payoffs:

$$\text{Public } c_1 > a_1, \text{ or } 3 > 2 \\ b_1 > d_1, \text{ or } 7 > 5$$

$$\text{Police } a_2 > b_2, \text{ or } 7 > 6 \\ d_2 > c_2, \text{ or } 7 > 4$$

Given these restrictions, Tsebelis shows (Theorem 1) that the only equilibrium outcome in this game is a mixed strategy outcome in which

$$p^* = (d_2 - c_2)/(a_2 - b_2 + d_2 - c_2), (1)$$

where p^* is the probability that the public speeds; and

$$q^* = (b_1 - d_1)/(b_1 - d_1 + c_1 - a_1), (2)$$

where q^* is the probability that the police enforce. The probability that the public does not speed equals $(1 - p^*)$, while the probability that the police shirk equals $(1 - q^*)$.⁴ Given the payoffs in Table 1, the equilibrium mixed strategies are $p^* = .75$ and $q^* = .66$; that is, in equilibrium the public speeds with probability .75 and the police enforce the law with probability .66, yielding expected payoffs of 3.63 for the public and 6.25 for the police. Given this equilibrium, Tsebelis shows that increases in the penalty for being caught speeding (i.e., a decrease in a_1) have no effect on the likelihood that the public chooses to speed (Theorem 2). The reason is that the increased penalty increases the likelihood that the police shirk (Theorem 3). Similarly, welfare measures (increases in c_1 or d_1) leave the crime rate unchanged but increase the likelihood of shirking by police (Theorems 4, 5).

Consider the effect of repeated play on police-public interactions. Suppose that the police and the public participate in an infinite series of one-shot games where the payoffs in each iteration t are given by Table 1. Thus, in the first iteration (itera-

**Table 1. The One-Shot
Police-Public Game**

Public	Police	
	Enforce	Shirk
Speed	$a_1 = 2, a_2 = 7$	$b_1 = 7, b_2 = 6$
Do not speed	$c_1 = 3, c_2 = 4$	$d_1 = 5, d_2 = 7$

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tion 0) the public chooses to speed or not to speed while the police choose to enforce or to shirk. Players then incur payoffs based on their strategy choices. On the second iteration, the players again face the same choices and possible payoffs. The series of one-shot interactions and strategy choices continue infinitely.⁵ Each player's payoff for the entire game is the (discounted) sum of its payoffs in each iteration of the game. Payoffs in iteration t are discounted by w^t , where $0 < w < 1$. Finally, on any iteration t players know the history of the game—the strategy choices made by each player in all previous iterations.⁶ These assumptions are standard features of iterated games (see Friedman 1986, chap. 3; Ordeshook 1986, chap. 3).

Tsebelis shows that if the police-public game is played once, the unique equilibrium mixed strategies are $p^* = .75$ and $q^* = .66$. However, the Folk Theorem suggests that if the game is iterated, other outcomes can be supported as equilibria. Consider the outcome where the public does not speed and the police shirk—the lower left-hand cell in Table 1. Both players receive a higher payoff in this outcome than they do in the mixed strategy outcome. For the public, $5 > 3.63$; for the police, $7 > 6.25$. This outcome is not an equilibrium if the game is played once. If the police shirk, the public prefers to speed, thereby increasing its payoff from 5 to 7. But what if the game is iterated? As I will now show, if w (the discount rate) is "high enough," the players can choose strategies that yield the no-speed, no-enforce outcome as a subgame perfect equilibrium. In other words, by coordinating their strategies, the players can make themselves better off.

Suppose the players use the following trigger strategies. Label the police strategy as *e-trigger* (*e* for *enforce*):

e-trigger $t = 0$: Shirk.

$t > 0$: Shirk if the public has

not sped in all previous iterations. Use the mixed strategy q^* otherwise.

That is, the police begin the game by shirking and shirk on subsequent iterations if the public does not speed. However, if the public ever speeds, the police respond by reverting to the mixed strategy where they enforce the law on each iteration with probability .66.⁷ Label the public's strategy as *n-trigger* (*n* for *not speed*).

n-trigger $t = 0$: Do not speed.

$t > 0$: Do not speed if the police have shirked in all previous iterations. Use the mixed strategy p^* otherwise.

That is, the public begins the game by not speeding and continues to do so as long as the police shirk. If the police ever switch to enforcing the law, the public responds by using the mixed strategy p^* on all subsequent iterations.

If the players use their trigger strategies, the no-speed, no-enforce outcome will be realized on each iteration of the game. These trigger strategies constitute a subgame perfect equilibrium if neither player prefers to switch from its trigger strategy to its maximin strategy (the strategy p^* for the public and q^* for the police), given that the other player responds by switching to its maximin strategy on subsequent iterations.

Consider first the public's incentive to switch. If both players use their trigger strategies, the public receives a payoff of 5 on each iteration, or a total payoff for the game of

$$5 + w^1(5) + w^2(5) + w^3(5) + \dots = 5/(1 - w).$$

If the public deviates from the agreement

with police and speeds on iteration 0 (or any iteration t) and plays its maximin strategy p^* thereafter, the police will respond by reverting to their maximin strategy q^* on the next iteration. In that case, the public receives a payoff of 7 initially and 3.63 on each iteration thereafter, or

$$7 + w^1(3.63) + w^2(3.63) + w^3(3.63) + \dots = 7 + w(3.63)/(1 - w).$$

Therefore, if the police use their trigger strategy, the public prefers to use its trigger strategy rather than deviate if and only if

$$5/(1 - w) \geq 7 + w(3.63)/(1 - w).$$

After some algebra, this condition can be expressed in terms of a minimum value for w : $w \geq .59$. Turning to the police, they receive a payoff of 7 on each iteration where they shirk and the public does not speed, or a total payoff of $7/(1 - w)$ if both players use their trigger strategies. If the police switch to enforcing the law and the public responds by reverting to p^* subsequently, the police receive a payoff of 4 initially and 6.25 on subsequent iterations, or a total payoff of $4 + w(6.25)/(1 - w)$. By inspection the police never have an incentive to deviate from an agreement to play trigger strategies: $7/(1 - w) > 4 + w(6.25)/(1 - w)$ for all w .

Therefore, the no-crime, no-enforce outcome can be realized as a subgame perfect equilibrium in the iterated police-public game with payoffs if players use their trigger strategies and $w \geq .59$. In contrast, the only equilibrium to the one-shot game is one where players use mixed strategies. The difference between these results must be stressed. Analysis of the one-shot police-public game finds there is some probability in equilibrium that the public will speed and some probability that police will choose to enforce the law.

However, in the iterated game examined here, if $w \geq .59$, the players are willing and able to coordinate their strategies to achieve an equilibrium outcome where the public does not speed and the police do not enforce the law.⁸

Adding iteration also modifies Tsebelis' conclusion about the effect of welfare measures. For example, suppose the public's reward for not speeding is increased: its payoff in the no-enforce, no-speed outcome is increased from 5 (as in Table 1) to 6. With this change, the player's equilibrium mixed strategies in the one-shot game are $p^* = .75$ and $q^* = .5$. If the players use these mixed strategies, the public receives a payoff of 4.5, while the police again receive a payoff of 6.25 on each iteration. However, if the players use trigger strategies to coordinate on the no-speed, no-enforce outcome, the public receives a payoff of $6/(1 - w)$. As a result, the public is better off coordinating with the police rather than deviating if $6/(1 - w) \geq 7 + w(4.5)/(1 - w)$, or $w \geq .4$. Remember that without the welfare measures, the value of w needed to sustain the no-speed, no-enforce outcome is .59. Therefore, in the iterated police-public game, welfare measures may reduce the crime rate by reducing the value of w needed to sustain an outcome where the public does not speed. In contrast, welfare measures have no effect on the equilibrium crime rate in the single-shot game (Tsebelis 1989, Theorem 4).

Tsebelis is exactly right in saying that previous analyses of criminal behavior have erred in using decision theory to model a situation that is game-theoretic in nature. I have drawn on the theory of iterated games to demonstrate an equally important point: if players interact repeatedly, it is inappropriate to make predictions about their behavior by analyzing a single interaction. Tsebelis' results are based on a game where the police and the public interact exactly once. As I have shown, the behavioral

predictions made by Tsebelis may not hold in an iterated version of the game. Therefore, empirical implications drawn from Tsebelis' analysis may not hold in the real world, where the police and the public interact repeatedly over time.

WILLIAM T. BIANCO

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George Tsebelis offers two results about bimatrix games (Theorems 6 and 7), both of which are false. Moreover, by focusing on two-person games, he fosters an incorrect view of game theory's implications. It is well known of course, that assessments of public policy based on simple decision-theoretic perspectives that fail to accommodate the interdependence of individual decisions can be misleading. For example, consider this event: a first-string star quarterback is injured; but his replacement, after calling a few perfunctory running plays, proceeds to pass more frequently than the injured star. Such an event might seem paradoxical, but only if we assume that people maximize expected payoffs against opponents whose actions (defensive tactics) are invariant with changing circumstances (such as quarterback injuries). If we suppose that the opponent also adjusts to such change and that everyone takes everyone else's adjustments into account, game theory resolves the paradox. In our example, passing more frequently with a less proficient player at the helm becomes understandable if we learn that the best response of the defense is to strengthen the line against the run so as to leave the offense with little option other than to pass.

Accommodating interdependent choice in two-person contexts, though, yields another apparent paradox that is the centerpiece of Tsebelis' analysis. Specifically, we find that the calculation of one player's equilibrium mixed strategy

depends only on the opponent's payoffs and vice versa, which suggests that if we wish to influence a person's mixed strategy, we should change the opponent's, rather than that person's, payoffs. If we are accustomed to thinking in decision-theoretic terms, this argument seems a strange disjuncture in reasoning. This disjuncture, though, is more apparent than real. First, the possibility of expressing a solution without reference to one's own payoffs arises only in two-person games. For games with $n > 2$ players, an equation stating the mixed equilibrium solution for one player must be formulated in terms of the remaining $n - 1$ players' strategies (thereby formalizing the game-theoretic reasoning that conditional on beliefs, each player chooses a strategy that is a best response to the strategies of everyone else); and, except for special cases, no equation can be solved without simultaneously solving for the strategies of all other players. In such simultaneous equation systems, all strategies are functions of each other. Thus, each strategy is a function of every player's payoffs. Only for two-player games are we guaranteed that the algebra of solutions reduces to the simple form upon which Tsebelis bases much of his analysis. Perhaps it is for this reason that game theorists find the algebra of two-person games to be little more than a curiosity rather than a basis on which to formulate public policy.

Second, the disjuncture identified by the algebra of two-person games is misleading even for that case because the connection with one's own payoffs is made through the concept of an equilibrium and the rationalizations for this concept's relevance. Thus, a particular strategy, identified by some algebraic identity, has behavioral meaning only because it is part of an equilibrium strategy pair and because the concept of an equilibrium is hypothesized to have meaning—and equilibrium pairs are a function of *both* persons' payoffs. Hence, it is only when we

**Table 2. Mixed Equilibrium Strategy
Zero-Sum Game I**

	b_1	b_2
a_1	1,-1	-1,1
a_2	-1,1	1,-1
a_3	2,-2	-2,2
a_4	-2,2	2,-2

pursue the shorthand calculation of mixed strategy equilibria and disregard the rationale of our calculations that an apparent disjuncture appears. Put simply, game theory is more than a set of algebraic relationships, and only when we take that theory as a whole can we give substantive interpretation to any piece of algebra. To suppose otherwise is to abuse that theory.

With respect, now, to technical errors, the first arises with respect to Theorem 6, which asserts, "If a two-player game has no pure strategy equilibria, it has a unique mixed strategy equilibrium." Aside from the fact that the essay, at this point, deals with $(n + 1) \times (n + 1)$ games, preconditions are unstated. However, the game's size is irrelevant, since we can expand a game without altering its strategic character by adding strategies that are strictly dominated by a preexisting strategy. Counterexamples to the asserted result, then, include the zero-sum game of Table 2. Row chooser has a mixed equilibrium strategy that gives positive probability only to a_1 and a_2 , an equilibrium strategy that gives positive probability only to a_3 and a_4 , a class of equilibrium strategies that gives positive weight to any three pure strategies, and another class that gives positive weight to all four of row chooser's pure strategies. Moreover, although column chooser has a unique mixed equilibrium strategy, $(1/2, 1/2)$, we can increase the number of equilibria for this player merely by reproducing col-

umns b_1 and b_2 under the labels b_3 and b_4 .

Tsebelis (p. 88) also asserts that games can be neatly subdivided into three classes: "(1) dominance solvable games and those with unique pure strategy equilibria, (2) games with multiple pure strategy equilibria, and (3) games without pure strategy equilibria (that have one, mixed strategy equilibrium)." However, in the following game, column chooser has an infinity of mixed equilibrium strategies ($1/2 \leq q^* \leq 4/5$, where q^* is the equilibrium probability that column chooser selects b_1); whereas row chooser selects a_3 with certainty, so an equilibrium can be a mixture of pure and mixed strategies, as in Table 3.

So that we should not suppose that counterexamples are limited to zero-sum games, consider the four-by-four non-zero sum game in Table 4. This game has at least two disjoint mixed equilibria, $[(1/2, 1/2, 0, 0), (1/2, 1/2, 0, 0)]$ and $[(0, 0, 1/2, 1/2), (0, 0, 1/2, 1/2)]$ that yield distinct expected payoffs as outcomes.

**Table 3. Mixed Equilibrium Strategy
Zero-Sum Game II**

	b_1	b_2
a_1	8,-8	4,-4
a_2	2,-2	7,-7
a_3	6,-6	6,-6

**Table 4. Mixed Equilibrium Strategy
Non-Zero Sum Game**

	b_1	b_2	b_3	b_4
a_1	4,2	2,4	-5,-5	-5,-5
a_2	2,4	4,2	-5,-5	-5,-5
a_3	-5,-5	-5,-5	4,2	1,8
a_4	-5,-5	-5,-5	1,8	4,2

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This example, moreover, also has a totally mixed equilibrium—a mixed strategy equilibrium that spans each player's entire strategy space—[(5/18, 5/18, 4/18, 4/18), (15/62, 15/62, 16/62, 16/62)]. (Notice that although this type of equilibrium is unique in this particular example, it is not unique in our first example.) In general, then, games can have a variety of mixed equilibria with different properties, and much of Tsebelis' discussion referring to the uniqueness of such equilibria requires restatement.

Problems also arise with respect to Theorem 7, which reads, "In a two-player game with no pure strategy equilibria, modification of the payoffs of one player will lead that player either to change the pure strategies that he or she mixed or to leave the mixed equilibrium strategy of the opponent." The clause "... will lead ... he or she mixed" is confusing; but a reasonable restatement is, "... will lead that player to change the set of pure strategies receiving nonzero probability from his or her mixed equilibrium strategy." This restatement presumes that we should first make certain that we do not change a player's payoffs in such a way as to render one or more strategies dominated, since then the successive elimination of dominated strategies can reveal a radical change in the game's strategic character. And although I do not otherwise quarrel with the assertion that one player's strategy will be unchanged by changes in the opponent's payoffs, the assertion that the opponent's strategy necessarily changes is false. For example, the theorem is false if we allow payoff changes in cells that occur with zero probability in equilibrium. And even if we suppose that mixed strategies span each player's strategy set, we must qualify matters by supposing that the theorem presupposes something other than linear monotonic changes, since solutions are invariant with such transformations (otherwise the rationalization for expected util-

Table 5. Two-Player Game with No Pure Strategy Equilibria

	b_1	b_2		b_1	b_2
a_1	—, 5	—, 3	a_1	—, 8	—, 6
a_2	—, 2	—, 7	a_2	—, 2	—, 7

ity calculations is invalid). Nevertheless, the theorem remains untrue without additional refinement.

Consider Tsebelis' equation 1: $p^* = (d - c)/[(a - b) + (d - c)]$, where p^* is the probability that player 1's first strategy is chosen in equilibrium and a , b , c , and d refer to player 2's payoffs. Simple manipulation yields $k = (d - c)/(a - b)$, where $k = p^*/(1 - p^*)$. Letting k be a constant, the equality is maintained if $(a - b)$ and $(d - c)$ are constant but we can change a and b or d and c . For example, if no pure strategy equilibrium exists, the mixed strategy solution for row chooser, (5/7, 2/7), is the same in both of the games in Table 5, where the numbers in the cells correspond to column chooser's payoffs. We cannot keep k constant, of course, if we change only one variable; but the fact remains that a valid result requires additional constraints on allowed payoff changes. However, at this point formulating and rigorously proving a correct theorem seems more effort than it is worth; and limiting discussion to two-person games not only precludes general relevance but also can yield misleading inferences. In any event, once we acknowledge the importance of accommodating interdependent choices into our analyses, we can refer to the considerable literature that treats the mathematical properties of solutions to games.

PETER C. ORDESHOOK

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In my article on the "Robinson Crusoe Fallacy" I made the point that using decision theory in the place of game theory may lead to incorrect conclusions, such as the belief that modifying a player's payoffs will modify its behavior. I used a two-by-two game to demonstrate that under certain conditions (the absence of pure strategy equilibrium), modification of one player's payoffs will lead to a modification of its opponent's strategy. The supporting game is not original and can be found in game theory textbooks (Luce and Raiffa 1957; Moulin 1981; Rasmusen 1989). The article's originality lies with the following three characteristics of its main result:

1. The main result holds under a wide variety of model specifications. In particular, it holds in models that assume parametric as well as strategic rationality, models that assume sequential as well as simultaneous moves by the players, and models that assume complete as well as incomplete information (one- or two-sided). Given that game theoretic models are usually highly sensitive to institutional or informational modifications, this stability of the equilibrium solution is remarkable indeed.

Of all these modeling variations, I consider the one in which the players are assumed to possess parametric rationality to be the most realistic because it does not assume that the two players—the police and the public in my example—are unified or that they have capabilities of coordination and strategic calculation. Each member of the police and the public maximizes his or her own payoffs given what the others do, and the equilibrium emerges as the outcome of all these independent decisions.

2. The main result is not an artifact of the two-by-two game. It holds for any finite number of strategies used by two players.

3. The main result is relevant to a wide variety of situations, including crime, international economic sanctions, hierarchies, regulation, and, in general, situations involving compliance to authority and monitoring.

Bianco's contribution to this controversy takes issue with item 1 by proposing another reasonable modification, leading to a different equilibrium. I find Bianco's underlying assumptions highly restrictive; and I will demonstrate that even if these assumptions are followed to the letter, an additional game-theoretic refinement leads to my solution. Ordeshook challenges items 2–3. He first makes some technical remarks concerning the bimatrix games in my article. This part of his argument focuses on item 2. I welcome his finding of technical errors, but they are minor and easily corrected by small verbal modifications. Ordeshook then makes a more general and substantive argument concerning item 3. He claims that "by focusing on two-person games, that essay fosters an incorrect view of game theory's implications." The major part of my answer responds to this contention, and I show why the arguments I made in the Robinson Crusoe fallacy article neither are mathematical artifacts nor lead to partial and incorrect conclusions but instead exhibit remarkable stability even in models where one introduces third players or incomplete information.

Bianco claims that if one assumes that the game between criminals and the police is iterated instead of single-shot, application of the Folk Theorem of iterated games suggests that the players can arrive at equilibria other than the one I indicated, and with different properties. Bianco focuses on a particular equilibrium, one where there is no crime and the police do not monitor. Investigation of the robustness of my results for iterated games is intrinsically interesting and empirically important because in certain

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situations (for example, small towns) the interaction between criminals and the police may resemble an iterated more than a single-shot game. I have no disagreement with Bianco's findings. However, I do find his extension of my model less relevant as a realistic model of crime than it may appear.

First, Bianco's argument follows. Consider the police and the public as unified players. By playing their mixed strategy equilibrium they forgo the gains of coordination. For example, they could agree not to violate the law and not to patrol unless one of them reneges, whereupon both would follow the strategies prescribed by the calculations of my article.

What are the behavioral consequences of such strategies? If coordination is possible, there is no crime and no enforcement. But if one player believes that the other will not forgo the gains of cooperation to punish a single deviation, it may deviate once to test his opponent. According to Bianco, crime will then have a frequency p^* and law enforcement q^* , exactly the values I gave in my article. Or if one player makes a mistake, the outcome that Bianco prescribes is the same as mine. Similarly, if a stranger comes into town and breaks the speed limit, the frequencies of crime and law enforcement presented in my initial article will prevail as a result of Bianco's argument, not mine.

The point is that if coordination among all the individuals comprising the two collective players fails, my equilibrium is likely to prevail. His solution (no crime and no enforcement) requires coordination among *all* individual players. How likely is this coordination? Is it a reasonable approximation of reality? The question of the realism of Bianco's argument seems to me very serious. It cannot, however, be raised about my model, because, as I show, evolutionary arguments (which make no assumption of strategic calculation or coordination) lead to the same equilibrium as the one-shot game where

the players are unified and rational.

Second, even if one makes the heroic assumption that coordination is possible, there are additional (more technical) problems with Bianco's argument. Bianco presents a numerical example leading to the equilibrium no crime and no monitoring (d_1, d_2 in the notation of my article). He notes that his result "should not be read too closely" (n. 8) and that different numerical examples would lead to different equilibria.

The reason that this equilibrium is possible in the iterated game is that it Pareto-dominates the single-shot game equilibrium; that is, it is to the mutual advantage of both (collective) players.⁹ Besides assumptions A1-A4 of my article, Bianco makes two auxiliary, implicit, and crucial assumptions in his numerical example: $d_1 > c_1$ and $d_2 > b_2$.¹⁰ By modifying these new assumptions, one can present any particular outcome of the initial game as an equilibrium in the iterated game. For example, if one makes the auxiliary assumptions that $b_1 > a_1$ and $b_2 > d_2$, (b_1, b_2) results as the equilibrium. If $c_1 > d_1$ and $c_2 > a_2$ are the auxiliary assumptions, (c_1, c_2) results as the equilibrium. Even the result of permanent violations and permanent enforcement (a_1, a_2) can emerge as an equilibrium outcome of an iterated game under the (implausible) auxiliary assumptions $a_1 > b_1$ and $a_2 > c_2$.¹¹

But regardless of auxiliary assumptions, the Folk Theorem leads us to expect an infinity of equilibria (Fudenberg and Maskin 1986). For example, if no crime and no enforcement is an equilibrium in the iterated game, so are all the outcomes where the public violates the law and the police enforce the law with any frequency smaller than the equilibrium frequencies in the one-shot game (p^* and q^*). The Folk Theorem is a possibility theorem. It says that practically any outcome is possible. As a result, it is a very poor guide for prediction when used alone.

This is why game theorists have offered additional refinements of the concept of (perfect) equilibria in iterated games. One such refinement is the concept of stationary equilibrium, which leads to the same behavior in each iteration of the game.¹²

As a result, stationary equilibria are the natural focal points when new players participate in the game. But in the police-public game, the equilibrium strategy pair in the single-shot game is the unique stationary equilibrium in the iterated game.

Ordeshook's technical points dispute item 2, that the analysis of the two-by-two game can be generalized for any finite number of strategies of the two players. I agree with Ordeshook's numerical examples and with their conclusions but question their relevance to his main point.

Let me start with an example. Consider the statement that a change in distance or time yields a change in velocity, velocity being distance divided by time. This statement is technically *false* because one can double both distance and time, thereby changing both, without changing velocity. However, the statement is true the rest of the time (that is, almost always), and any argument including it needs very little substantive modification to prevent falsification along these lines. Ordeshook's last example is exactly of this kind (it holds only because of the specific numerical values he uses); and the rest of his numerical examples share similar features, that is, technically they falsify some of my statements but leave them largely intact substantively.

Ordeshook is correct that a game without pure strategy equilibrium has, in general, multiple mixed strategy equilibria (see Harsanyi 1973b). However, all these mixed strategy equilibria, contrary to Ordeshook's assertion that they "have different properties," share a common property that lies at the heart of my argument; namely, each player's equilibrium strategies depend on its opponent's payoffs. As a result, a change in one player's payoffs

will in general either destroy the mixed strategy equilibrium or lead the opponent to modify its strategy (as I demonstrated in my article). It is also possible on occasion that changes in the payoffs may be of a special numerical value such that even the opponent will not have to modify its strategy (changes in the payoffs of strategies mixed with probability zero or linear transformation of all payoffs of one player, to use Ordeshook's examples). However, Ordeshook's examples rely on particular numerical modifications and do not affect the general validity of my argument.

Concerning the partition of two-person games, Ordeshook presents an interesting example with an infinity of mixed strategy equilibria. This example is, again, exceptional. In game-theoretic terms, the equilibria Ordeshook presents are irregular.¹³ However, it can be shown that in "almost all normal form games all equilibria are regular (hence, quasi-strict, strongly stable, essential, isolated, strictly proper, proper and perfect)" (Damme 1987, 41).¹⁴ Damme explains why irregular equilibria are exceptional: "The existence of an irregular equilibrium entails a special numerical relationship among the payoffs of the game, and this relationship can be disturbed by perturbing the payoffs of the game slightly."¹⁵ Consequently, my classification holds for "almost all" normal form games. "Almost all" normal form games can be partitioned into games with (1) multiple pure strategy equilibria, (2) one pure strategy equilibrium, or (3) no pure strategy equilibrium (which, as Ordeshook points out, will have one or more mixed strategy equilibria).

Thus, Ordeshook's technical points are correct. However, because of their exceptional character, they do not modify the substance of item 2, that the Robinson Crusoe fallacy is not an artifact of a two-by-two game. His major point, however, is that because my results are derived from two-person games they are "little

more than a curiosity rather than a basis on which to formulate public policy." If correct, this assertion would be fatal for my argument.¹⁶

How reasonable is a two-person game as a modeling tool to understand social or political situations? It obviously depends on the situation one wants to model. It also depends on the state of the art in a particular field, that is, on the other tools available. In my article I claimed that the Robinson Crusoe fallacy was relevant to cases such as international economic sanctions or regulation, where the number of players is either actually two or very restricted and where, as a result, such an abstraction is legitimate. Ordeshook does not address this point. Instead, he focuses on the model's application to the problem of crime, where the large number of actors involved makes the assumption of a two-person game more problematic. Though partial (that is, addressing only one of the applications of my argument), his question is justified by the social importance of the crime issue. Before proceeding, however, let me clarify that the state of the art in the field is such that the alternative to my two-person game approach to crime is not Ordeshook's multi-person game approach but a one-actor decision-making model, which arguably is not a step forward but a step backward.

By way of a positive defense of my model, I shall introduce some additional actors, such as the legislature and the courts. I shall show that under reasonable modeling assumptions in this multiperson game the logic of my model remains unchanged, contrary to Ordeshook's expectations. In particular, I show that the modification of the payoffs of one player does not affect its behavior but (in general) induces changes in the equilibrium behavior of his opponent. I shall then complicate the model further and introduce even more actors under the form of different types of police and of publics. I shall use games with incomplete informa-

tion to show that the conclusions of my initial article hold not only qualitatively (the equilibrium has the same properties as the equilibrium of the initial article) but also quantitatively for the particular model I present here; that is, the equilibrium is exactly the same as in the initial model. Finally, I shall use this incomplete information model to show how the two-person game approach can generate further interesting insights into political and social situations. I hope that this evidence will put Ordeshook's objections to rest.

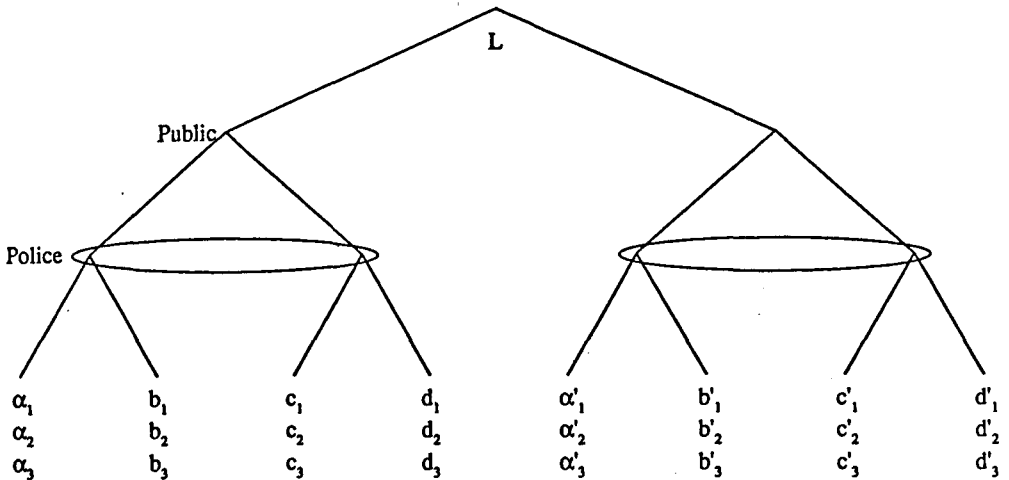
Introducing Third Players: The Legislature or the Courts

The police-public game is part of a more complex game where a third player, the legislature, makes the rules. I will introduce the legislature into my model as an explicit third player and show that contrary to Ordeshook's expectations, both its logic and conclusions remain intact.

The role of the legislature is to vote on different laws regulating penalties for illegal activities (and possibly the rewards of legal activities) and to create an incentive system for the police to enforce the law. In game-theoretic terms, the legislature *ex ante* decides the payoffs of the police and the public. Figure 1 represents a more complicated game where the legislature moves first and selects between two different sets of payoffs for the police and the public or sets the rules of the police-public game. In normal form this game can be represented by a two-by-four-by-four table, where the legislature has to choose between two strategies while the police and the public have four strategies each.¹⁷

This is a three-player game and, in general, as Ordeshook says, one should expect that modification of the payoffs of one player affects its behavior as well as that of the other players in the game. However, additional reflection suggests that this game has a particular form due

Figure 1. Game among Legislature, Public and Police:
Legislature Moves First



Assumptions: $c_1 > \alpha_1, b_1 > d_1, \alpha_2 > b_2, d_2 > c_2$
 $c'_1 > \alpha'_1, b'_1 > d'_1, \alpha'_2 > b'_2, d'_2 > c'_2$

to the fact that the legislature moves first, setting the rules for the remainder of the game, whereas the public and the police move only after they know these rules. For this game, application of standard game-theoretic rules leads to the following solution (following Selten 1975). The legislature knows that whatever its choices of rules, the police and the public will play the equilibrium strategy pair calculated in my Robinson Crusoe article. It can therefore calculate its costs and benefits from each set of rules and select whatever rules are optimal for itself. Subsequently, the police and the public will play whichever game the legislature selected, as prescribed in my article. Therefore, in this three-person game, modification of the public's payoffs will (in general) modify the equilibrium behavior of the police, and vice versa.

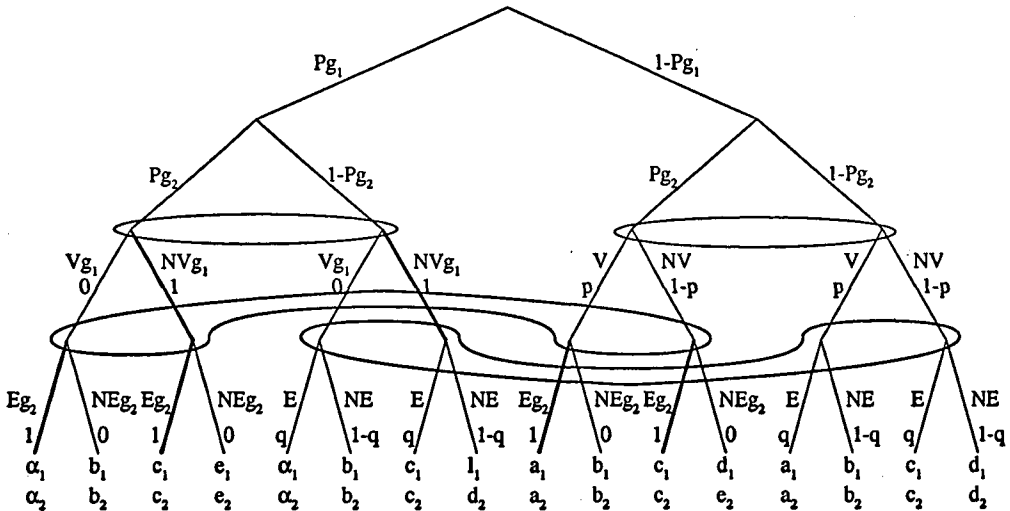
Note that this is not the only equilib-

rium of the game; and for the remaining mixed strategy equilibria Ordeshook is right—modifying the payoffs of the public or of the police will (in general) have consequences for the behavior of both players. However, in the presence of multiple equilibria standard game-theoretic techniques require one to search for an equilibrium with additional properties of stability. The equilibrium I have calculated is the only subgame perfect equilibrium of the three-player game, and it has exactly the properties described by the main result in my article. What drives this more complicated three-person game to the same outcome as the simpler two-person game is the fact that the third player moves *before* the other two.¹⁸

The same arguments can be replicated if one brings in other players who move *after* the police and the public have selected their strategies: lawyers, courts,

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Figure 2. Game between Public and Police: Incomplete Information
(each player has two possible types)



Assumptions: $c_1 > \alpha_1$, $e_1 > b_1 > d_1$, $\alpha_2 > b_2$, $d_2 > c_2 > e_2$
Dominant strategies are indicated by heavy lines

the prison system, and so on. Again, application of the appropriate game-theoretic concepts leads to the results presented in my article. As long as the dyadic interaction of the police and the public remains intact and the other players move either before or after those two players, my results hold. The next question is, of course, what happens if other actors move simultaneously with the two main players?

Introducing Different Types of Police and Publics: The "Naive" and the "Sophisticated"

I cannot see how any other distinct player could participate simultaneously in the police-public game. However, a more realistic representation would make use of different kinds (or "types") of police agents or members of the public participating in the game. These different members of the public and the police have different payoffs and when they interact

with each other they receive the payoffs corresponding to their mutual choice of strategy and their type. As a first step in this approach, imagine that there are two types of public: good citizens, who never violate the law, and bad ones, who prefer to violate the law when the police are not around. The first have payoffs making compliance with the law the dominant strategy; I will call them "naive." The second have payoffs similar to those in my Robinson Crusoe article; I will call them "sophisticated." Similarly, imagine that there are two types of police agents: "good" ones, who always monitor, and "bad" ones, who monitor only when the public violates the law. The first have payoffs making monitoring the dominant strategy; I will call them "naive." The second have payoffs like the police in my Robinson Crusoe article; I will call them "sophisticated." Imagine also that each member of the public and each police agent knows his or her own payoffs (his or her "type") and the proportion of naive

and sophisticated players in the general population but does not know the type of opponent being faced.

Figure 2 represents a game of incomplete information modeling the situation I have just described. A fictitious player ("nature") selects whether a particular member of the public will be naive (with probability p_{g1}) or sophisticated (with probability $1 - p_{g1}$). Similarly, nature chooses whether a particular police agent will be naive (with probability p_{g2}) or sophisticated (with probability $1 - p_{g2}$). Each player then chooses one of two strategies.¹⁹ The payoff for each player associated with each outcome is found at the final nodes of the game tree, and the order of these payoffs is presented in the margin of Figure 2.

Although there are two different police agents and two different members of the public, the use of incomplete information makes the game in Figure 2 technically a two-person game with the same property as the simple two-by-two game presented in my article: modification of one player's payoffs induces the other to change equilibrium strategies (in general).

In the two-person game in Figure 2 each player has four strategies, so that the game has multiple mixed strategy equilibria. However, in only one of these equilibria the players do not use dominated strategies. For this equilibrium, the naive players (police and public) always use their dominant strategies, while the sophisticated members of the public violate the law with frequency

$$p^* = (d_2 - c_2) / [(1 - p_{g1})(d_2 - c_2 + a_2 - b_2)], \quad (3)$$

and the sophisticated members of the police monitor with frequency

$$q^* = \{(b_1 - d_1) - [p_{g2}(b_1 - d_1 + c_1 - a_1)]\} / [(1 - p_{g2})(b_1 - d_1 + c_1 - d_1)]. \quad (4)^{20}$$

Observation of equations (3) and (4) leads to the conclusion that in this (more complicated and more realistic) game, too, modification of the payoffs of one player leads the opponent to modify strategy (in general). This is a general property: as long as the number of police agents and members of the public is finite, the equilibrium strategy pairs will exhibit this property.²¹

Comparison of equations (3) and (4) with the equilibrium strategy pair of the simple two-by-two game in my article (see Bianco's equations 1-2) leads to the conclusion that the two games have exactly the same equilibria. In other words, as long as sophisticated players use mixed strategies, the frequency of crime and of law enforcement that the two models predict is exactly the same!²² Although this identity of equilibria is not a general property, it generates some interesting insights.

The Congestion Effect

The reason that the equilibria of the two models (that with incomplete information and that presented in the initial article) are identical follows. Since by assumption the naive players have a dominant strategy, they will select this strategy regardless of other considerations. The sophisticated types, however, calculate their optimal strategy as a function of the optimal (equilibrium) strategy of their opponent, as well as the frequency of their own and their opponent's types. So, at equilibrium, the frequency of violation of the law is a function not only of the payoffs of the opponent but also of the frequencies of the different types of players. Simple observation of equations (3) and (4) suggests that the more law-abiding citizens there are, the more the non-law-abiding feel free to violate the law. Similarly, the more reliable police agents there are, the more the other police agents will free ride.

The conclusion of this brief discussion

is that the existence of identical police agents and members of the public (i.e., having identical payoffs) is not a necessary condition for the results I presented in my article. Even if parts of the public and the police have a dominant strategy and only one part of the players have the order of payoffs I presented there, the equilibrium behavior of the players will approximate the equilibrium calculated in my article.

I have often heard the following objection to the supposed lack of realism of the Robinson Crusoe fallacy assumptions: "I would not commit robbery even if I knew that the police were not around" or "Not all police agents stop patrolling if there is no crime." Such objections do not directly address the arguments made in the article, since I do not claim that all members of the public or all police agents have the preferences described by inequalities A1-A4. However, the incomplete information model presented here answers such objections directly. It does not matter whether all members of the public or all police agents can be described by A1-A4. As long as there is a sufficient number of them, the equilibrium behavior will be the same.

In a seminal article, Haltiwanger and Waldman (1985) examine what they call congestion effects, that is, situations where each person is made worse off, the greater the number of others who make the same choice. They prove that in such cases the existence of some sophisticated participants (that is, those who can on average anticipate outcomes) will produce the same outcome as if all participants were sophisticated. Haltiwanger and Waldman use the job market as an example of their argument: if some agents can correctly anticipate that there will be an excess of doctors in the years to come, they will choose other professions, generating an equilibrium approximating the situation where all agents could guess the future correctly.

Crime exhibits properties similar to

those characteristic of congestion effects. The two types of police and public in my example do not differ in their predictive capacities but rather in their capacities to adopt different strategies: one type of each player can adopt only one strategy, while the other can make a choice according to prevailing conditions. And it is this ability to engage in strategic choice that drives the more complicated model with incomplete information to the same equilibrium outcome as the simple two-by-two model.

The previous discussion suggests that two-person games, if used appropriately, are not mathematical curiosities irrelevant for policy, as Ordeshook claims, but flexible and productive intellectual tools. Moreover, Ordeshook's claim that "limiting discussion to two-person games not only precludes general relevance but also can yield misleading inferences" is not relevant to my analysis. First, it is not relevant because two-person games may be subgames of more complicated multi-person games; in this case, any subgame perfect equilibrium by definition prescribes equilibrium behavior in the subgame. Second, it is not relevant because by using incomplete information games with more than two physical players can sometimes be represented as two-person games.

These are some of the reasons why, contrary to Ordeshook's assertions, two-person games *have* been used by game theorists for analysis of complicated situations and policy prescriptions. Examples include tax policies (Graetz, Reinganum, and Wilde 1986), regulation (Baron and Myerson 1982) and government policy (Kydland and Prescott 1977).

To conclude, Bianco's and Ordeshook's helpful criticisms and thoughtful objections suggest some additional modeling possibilities that I had not considered and highlight some new implications of the Robinson Crusoe fallacy article. In particular, Bianco's comments led me to see

that the equilibrium I had calculated is also the only stationary equilibrium of an iterated game; and Ordeshook's remarks, along with pointing out some mistakes and exceptions to some of my claims, helped me realize that the initial simple model had more applications than I had anticipated.

However, it would be a mistake to conclude that all game-theoretic models of crime would necessarily lead to the same result as that presented in the Robinson Crusoe article. Additional modifications and complications that increased the realism of the model could alter its conclusions, just as my argument modifies the conclusions of the economic approach to crime.

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Notes

William Bianco thanks John Aldrich for comments on an earlier version of this essay. George Tsebelis thanks Jeffrey Frieden, Miriam Golden, Eric Rasmusen, Thomas Schwartz, and John Zaller for comments.

1. For a notable exception, see Hamburger 1979, 71-74.

2. See especially his discussion of how the players will respond in the short run and the long run to changes in their payoffs (Tsebelis 1989, 82).

3. According to Friedman (1986, 103), the Folk Theorem is due to Robert Aumann. Aumann (1981) suggested that since the late 1960s, game theorists have believed that any individually rational outcome could be supported as an equilibrium, given no discounting of future payoffs. Aumann labeled this result as the Folk Theorem because its source was not known. Subsequent work (summarized in Fudenberg and Maskin 1986) has proved the result and generalized it to iterated games with discounting.

4. For a discussion of whether this claim is generally true for bimatrix games with no pure strategy equilibria, see Ordeshook's contribution to this controversy.

5. Games of infinite iterations with discounting are mathematically identical but analytically more tractable than games where players interact for an indefinite number of iterations. It is the latter case that corresponds to real-world interactions between police and public.

6. This information requirement is nontrivial but does not seem terribly severe. In real-world terms, one might imagine that the public can see whether the police are enforcing the law or shirking by observing whether the police are parked beside the road, radar guns in hand, or outside the local coffee shop. Similarly, shirking police can determine whether citizens are breaking the law (at least in qualitative terms) by noting the speed of cars as they drive by the coffee shop.

7. The Folk Theorem suggests a focus on the trigger strategy where any deviation from coordination triggers "permanent retaliation" against the player who deviates. Intuitively, if this threat does not deter defection, no other threat can do so (for a further discussion, see Friedman 1971).

8. This result should not be read too closely. Player's single-iteration payoffs can be specified such that no outcome dominates the outcome where players use their mixed strategies p^* and q^* on each iteration. Some configurations of payoffs allow additional outcomes to be realized as equilibria in the iterated game: if $a_1 = 2$, $a_2 = 8$, $b_1 = b_2 = 7$, $c_1 = 3$, $c_2 = 2$, $d_1 = 6$, and $d_2 = 3$, the outcome where the citizen speeds and the policeman shirks on each iteration is an equilibrium, given appropriate strategies and a high enough w . Under incomplete information, other outcomes might be realized as equilibria in the iterated game given "reputation effects" (Kreps and Wilson 1982).

9. In the single-shot game, the public gets $EU_p = (b_1c_1 - a_1d_1)/(b_1 - d_1 + c_1 - a_1)$, and the police get $EU_{pol} = (a_2d_2 - b_2c_2)/(a_2 - b_2 + d_2 - c_2)$.

10. The reader can verify that under these assumptions and the assumptions A1-A4 of my article, d_1 and d_2 are greater than the equilibrium payoffs for the single-shot game (see n. 8).

11. The first equilibrium prescribes permanent crime and no enforcement and might be reasonable for crimes like jaywalking; the second equilibrium prescribes the permanent enforcement and no crime and might be reasonable for crimes like murder.

12. More precisely, in "every structurally equivalent subgame" (see Baron and Ferejohn n.d.). For a general discussion and dispute of Folk theorem arguments, see Guth, Leininger, and Stephan 1988. Their arguments lead to the adoption of stationary equilibria as the appropriate solution concept in iterated games.

13. The equilibria are irregular because they are not isolated; that is, one can find other equilibria in the neighborhood of each.

14. The definitions of these terms can be found in Damme 1987 and are not essential here. However, in a deeper sense, these properties of stability of Nash equilibria in normal form games are the reason why the different models I presented in my article lead to the same equilibrium. It has been shown that Nash equilibria are the only rational solutions to simultaneous games (Bacharach 1987; for further discussion see Tsebelis 1990b, chap. 2). Therefore, if the

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only equilibria of a game are in mixed strategies, they will (in general) exhibit all the desirable properties of stability. For example, since they are regular, they can be shown to be the limit equilibria in games with incomplete information and perturbed payoffs (Harsanyi 1973a). Similarly, they are likely to be evolutionary stable.

15. In more precise terms, from all games with pure strategy spaces $S_1, \dots, S_n$, the set with an irregular equilibrium is closed with Lebesgue measure zero (see Harsanyi 1973b or Damme 1987, 42–43 for the proof). In plain English, the probability that a two-by-three game chosen at random will have an equilibrium like the one in Ordeshook's example is zero.

16. Ordeshook makes one additional point concerning the necessity to "take [game theory] as a whole" in order to "give substantive interpretation to any piece of algebra." I assume he means that equilibrium strategies have to be considered in pairs. I agree with him, but I cannot see what he finds objectionable in my treatment. I compare two games with the same payoffs except for the size of the penalty (a_i) and observe that the difference between their equilibria is the frequency of police enforcement, not the frequency of crime. This is standard comparative statics analysis.

17. The public can choose to violate the law regardless of the choices of the legislature, can violate the first set of rules but respect the second, respect the first set of rules but violate the second, or always respect the law. Similarly, the police have the choice among four strategies.

18. See Selten 1975 and Myerson 1978. As a result, each of the initial two-person games is technically a subgame of the three-person game, and the subgame perfect equilibrium concept is applicable.

19. "Good" police agents always choose to monitor and "good" citizens always choose to abide by the law.

20. To derive these two equations one has to write down the expected utility of each player as a function of the payoffs and the probabilities p (that the sophisticated member of the public violates the law) and q (that the sophisticated police agent monitors) and set the first derivatives with respect to p (for the public) and q (for the police) to zero (Tsebelis, n.d.a).

21. For an infinite number of types of players, this proposition is not true. For the solution of a similar problem where the number of types is infinite and the different types follow a uniform distribution, see Tsebelis 1990a.

22. The reader can verify this statement by calculating the overall frequency of violation of the law as the product of the frequency of sophisticated members of the public and the frequency that this sophisticated public violates the law (equation 3). Similarly, the frequency of monitoring is equal to the proportion of naive police agents and the prod-

uct of the proportion of sophisticated police and the frequency of monitoring (equation 4).

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CAN POLITICAL SCIENCE HISTORY BE NEUTRAL?

In the December 1988 issue of this Review, John Dryzek and Stephen Leonard argued the need for "context-sensitive" histories of the discipline of political science. In their view, disciplinary history must guide practical inquiry if it is to be most useful. The course of their argument draws the criticisms of three political scientists concerned about the history of political science—James Farr, John Gunnell, and Raymond Seidelman. Dryzek and Leonard respond to their critics and underscore their own rationale for enhanced interest in the history of the discipline.

Spurred on by a spate of recent histories of political science, John S. Dryzek and Stephen T. Leonard (1988) have helped to raise the level of metatheoretical and historiographical discourse about our discipline. While critical of those histories (which they divide exclusively into *Whig* and *skeptic*), they have been principally concerned to encourage the practice of disciplinary history because of their constructive thesis that our discipline's identity depends in good part on its past and on how we write the history of the past. About this, they assert, we can never be neutral. They have even gone some distance toward elaborating the outlines of a postempiricist historiography for our discipline, one that stresses progress, diversity, and contextual understanding. There is much in this with which one can sympathize.

Yet there are also a number of matters that either invite criticism or simply go unmentioned. I am not convinced by the alleged exclusivity between *Whig* and *skeptic*, and certain figures in the history of political science belie the distinction. I am also not convinced by their claims about the impossibility of *ever* being neutral with respect to narrating the history of our research traditions. Beyond

research, there is no discussion of the public standing of political science, especially as regards civic education—though this is essential for any general thesis about the history and identity of our discipline. In the main, however, my concern is much the same as theirs—to encourage the resurgent interest in the history of our discipline and to facilitate such conversations with past political scientists as take both their contexts and our concerns seriously.

Dryzek and Leonard correctly characterize their position as postempiricist. They adopt a Lakatosian (1978)—and I think a Laudanian (1977)—perspective not only about problem solution as the principal criterion for understanding scientific progress within research programs or research traditions but also about the intimate connection between the history and philosophy of science. There is also the concern for, as Lakatos puts it, "tolerance and tenacity" (Lakatos 1978).

Dryzek and Leonard press their case for this postempiricist position neither by attending to any particular figures in the history of political science nor by evaluating any particular research program, past or present. This is somewhat curious and a bit disappointing since this is precisely

what their position requires. (But an article—especially a programmatic one—cannot do everything, and only time will tell whether the prescribed way of proceeding will yield fruit.) Instead, Dryzek and Leonard press their case by critically commenting on the work of a number of recent historians of political science, most notably Collini, Winch, and Burrow (1983), Ricci (1984), Seidelman and Harpham (1985), Janos (1986), and a few others from the less-recent past, like Easton (1953) and Merriam (1925). These histories are divided into “two mutually exclusive categories”: *Whig* and *skeptical* (p. 1253). “Between these two stances,” we are told, “there would appear to be no others. Nor should we be surprised by this” (p. 1255). The Whig is intent on vindicating the present (or the future) state of political research by writing a teleological history of the past as leading up to our present (or future) glories. The skeptic is intent to criticize the present state of the discipline by urging an abandonment of the recent past in order to be politically relevant to a public world outside the academy.

The histories categorized in these two ways may well be discussed differently and certainly in less categorical terms (as I have tried in Farr 1988a). But here I want to suggest that the categories of Whig and skeptic—however convenient for purposes of exposition—are not “mutually exclusive,” at least if we use these terms broadly enough to do justice to their range of historical reference. Here it will help to introduce into the conversation two historical figures—David Hume and Adam Smith—not only because they are among the first of the modern “political scientists” (to judge by their own terms) but also because they have been best described as “skeptical Whigs” (Forbes 1975). In this way, Hume and Smith are directly relevant to the claims Dryzek and Leonard make, down to the *very terminology* they use to describe contempo-

rary debates about the historiography of our discipline. Though the late eighteenth century and the late twentieth century provide very different contexts, here past political scientists in our history can inform the present and its historiography. While Hume and Smith help us challenge the Whig-skeptic categorization, they do so in a way that directly follows from Dryzek and Leonard’s argument.

In terms of political judgments, Hume and Smith were “skeptical Whigs” because, though Whigs in terms of policy, they dared to be skeptical about certain of the verities of English Whiggism since the Glorious Revolution, including British liberty and the matchless English constitution. As Duncan Forbes puts the matter, this political judgment has a “scientific” foundation, for it was a consequence of “applying ‘experimental’ philosophy to morals [and] observing political phenomena with the cool detachment and neutrality of the impartial spectator” (1975, 180). Refusing to accept the self-congratulating presentism of “vulgar Whiggism” or the romantic historicism of Tory or Jacobite reaction, “Smith, like Hume, was a ‘skeptical’ Whig because he was a political scientist, or ‘philosopher’ in politics” (p. 180). Moderation and balance were key virtues, as were respect and tolerance. Indeed, progress in the arts and sciences was possible because of these virtues, as was a “sympathetic” understanding of earlier times.

Skeptical Whiggism in this sense was evidently a political position tied to the claims of science. But it was also a *historiographical* one, as well. The latter can be seen in Smith’s *Wealth of Nations*, where he speaks not only about the progress of modern civilization but also about freedom “in our present sense of the term” (1976, 423). There is the even more striking balance between progress and historical relativism in Smith’s phenomenal essay, the “History of Astronomy” (1980, 31–105). Similar historiographical impli-

cations can be seen in the framing of Hume's *History of England*, and in his contextualized appreciation of Machiavelli and of himself. Machiavelli, Hume maintained, was one of "the best historians . . . among the moderns"—whose limitations were not those of method or intelligence but only those of his having "lived in too early an age of the world" (1826, 98). Even now, Hume suggests, "the world is still too young" and we may entertain a "suspicion of still further changes" (1826, 100).

Dryzek and Leonard, it would appear, are themselves skeptical Whigs (in the historiographical sense). Like Hume and Smith, they are political scientists who want to hold on to progress but with attention to historically relative contexts. They also wish to encourage pedagogically useful histories, even as they criticize and stand above the fray of extremist or irrelevant historiographies. This different categorization of historiography is one consequence of bringing Hume and Smith into the conversation, and of allowing terms that describe them to replace the false antinomies suggested by Dryzek and Leonard. But some other implications seem to be at stake here, as well.

If Hume and Smith are right about the sort of detachment and moderation we can have as historians or political scientists, at least over some matters some of the time, it would appear that we might allow a greater degree of neutrality in writing the history of research traditions in political science than appears to emerge from Dryzek's and Leonard's discussion. Although neutrality is doubtless impossible with respect to how one does history at all, it seems that we can be rather *more neutral* with respect to such diverse things as the appropriation of individual figures in research traditions, the professional identity of our discipline, the actual practices of inquiry, and even contemporary political ideology. In the example at hand we might appreciate this by considering

that the (allegedly) moderate historiography of Hume and Smith may well be nonneutral, even partisan, as regards other (allegedly extremist or irrelevant) historiographies but remain relatively neutral toward the theories and figures it narrates and much else besides. For example, Smith and Hume took a decidedly nonneutral epistemological and political stand against "enthusiasts" (see Farr 1988b). But they remained rather more neutral as regards Aristotle versus Hipparchus (as astronomers) or Machiavelli versus Polybius (as theorists). The identity of their discipline (or rather their discourse) of political science did not seem to be at issue or on the line in any direct or serious way when Hume and Smith told the stories of *these* particular figures.

Compare this with what I take to be an implication of Dryzek's and Leonard's brief mention of pluralist political science and Whig historians of figures like Arthur Bentley during the 1950s. Whigs, as credited by Dryzek and Leonard "do engage in what we have argued is the essential task of reconstructing research traditions, thus providing historical warrants for the triumph of their favored approaches. . . . But as the present changes, so will its precursors, and Whigs will have to continue rewriting history. Thus Bentley comes to prominence as a precursor of pluralist political science in the 1950s—and fades along with pluralist interpretations in the 1960s and 1970s" (p. 1255). Taking a hint from the practice of Hume and Smith, I want to suggest that the historian takes a nonneutral stance toward other historians (say, skeptics). But with respect to Bentley himself or to the fine-grained research of contemporary political scientists, the historian may remain rather more neutral. Indeed, the story of Bentley might be all the better *as history* to the extent that the identity of pluralist political science is *not* at issue. I take it as some corroboration of this view that the best, most contextually sensitive histori-

cal study of Bentley—James Ward's (1984)—appeared 15 or so years after the alleged fading of Bentley's prominence and of pluralism itself. Ward's (non-neutral) battles were with other disciplinary historians, like Kress (1973) and if drawn out could be engaged by, say, Whigs, Straussians, Gadamerians, or Foucaultians. But as regards Bentley or pluralistic political science—or the contemporary prospects of rational choice theory, the new institutionalism, agent-structuralism, or a whole host of competing research programs—Ward did and could in good faith remain rather more neutral.

From this I draw the conclusion that we want to encourage more histories of political science, *whatever* their form, content, or historiographical posture. But these may well be rather more agnostic about (or neutral toward) the otherwise pressing debates about the research identity of our discipline and much else besides. And they need not—though of course they may—attempt to determine the progressiveness of the problem-solving capacities of this or that research tradition in political science. If Dryzek and Leonard are skeptical Whigs in the historiographical tradition of Hume and Smith, there are yet other kinds of skeptical Whigs and even altogether different kinds of disciplinary historians concerned with the traditions of research in political science.

But even this seems an insufficient conclusion when we move beyond research in order to consider the educational mission of political science. Unfortunately, Dryzek and Leonard are silent about this. Yet there is a veritable tradition—whose history must not be forgotten (and indeed must be told to sustain their thesis)—that not only stresses education as such but even identifies political science with civic education. This, at any rate, seems to be one of the messages of Hume and Smith's (and numerous others') "science of politics." They conceived of civic educa-

tion as an important aspect of modern state policy necessary to inculcate the appropriate attitudes of moderation and civic virtue (which they alleged were the very attitudes of political science itself). Well after the passing of those early skeptical Whigs, U.S. political scientists continued this tradition, as can be seen in the course of the nineteenth century in the writings of Francis Lieber, John W. Burgess, Theodore Dwight Woolsey, Woodrow Wilson, and others. Anna Haddow's *Political Science in American Colleges and Universities* (1939) documents some of this, as does Bernard Crick's (1959) critical accounting, *The American Science of Politics*. This tradition lives on well into the twentieth century, to judge by the efforts of, for example, John Dewey (1916) or Charles E. Merriam (1931, 1939). To take half a century in a jump (and to skip much in this tradition), the recent studies by Amy Gutmann (1987) and Richard Battistoni (1985) are helping to keep this discussion alive and pressing for the present state of our discipline.

It would seem, then, that the identity of political science cannot be adequately debated without considerable attention to civic education, this defining feature of the history of our discipline. Here the stakes seem very high, decidedly non-neutral, and rather different from the stakes that implicate our research traditions. In "Political Science and Political Education," Norman Jacobson once suggested just how high and how different: "Whether we wish it or not, as American political scientists we today wear the toga of the Legislator, to instruct half the nations of the globe. By giving them our science, we risk giving them our laws as well. In this perilous state, we could do worse than to recall the warning of Rousseau, that giving laws to men is the prerogative of a god, and bears the responsibility of a god" (Jacobson 1963, 569). Some political scientists might well wish

to shrink from accepting the burdens of divinity. But none of us can avoid the responsibility of addressing questions about our civic and pedagogical identity in history. Even the familiar and often painful debates that go on in department after department about whether we are principally researchers or teachers have their (significant? trivial? noble? shameful?) place in a history of political science as civic education.

I cannot imagine that Dryzek and Leonard would dispute the importance of this topic—had they written at greater length they doubtless would have discussed it. Raising it, however, requires that we pay attention not only to the research programs and the theories of political science but also to its educational practices and public functions (and if Jacobson [1963] is right, this takes us beyond our own national borders, as well). Here the postempiricist concerns with contextualized problem-solving capacities may have some place. But a host of other concerns come in to play, as well, including decidedly political ones—about liberalism and democracy, authority and freedom—that take the disciplinary “conflicts and debates” (p. 1258) out into the public sphere. The past, present, and future identity of political science must be found or forged in a broader and more public history about the education of citizens in our republic.

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Although the general thesis advanced by Dryzek and Leonard is relatively straightforward, the supporting argument is unclear, and a critical response cannot be presented without first offering a rendition as a benchmark. I find myself in agreement with the spirit of the article but dubious about its reasoning.

The authors ask what disciplinary his-

tory can contribute to the “identity, practice, and progress of political science.” Probably few would quibble with their conclusion that historical reflection is in some way constitutive of identity; but it is considerably more contentious to claim that a less presentist historiography, eschewing the extremes of Whiggism and skepticism, can provide “guidelines for research” and intersubjective measures of disciplinary progress. I do not, however, want to dispute this general conclusion as much as disentangle and examine the claims on which it rests. Many of these arguments are informed by undefended assumptions about the history and philosophy of natural science and reflect a failure to address some important and basic internal and external features of the history of political science.

One central argument is that histories of natural science are different from those in political science in that the former are “hegemonic” and the latter are “multiple.” This is because of the “socially constituted and historically determined nature of the objects of political inquiry.” At any time natural scientists tend to subscribe to the same historical account, which is largely a story of progress told from the perspective of a dominant theory and identity. Political science, however, has plural identities and perspectives on the past that are sustained by the fact that the subject matter is historically mutable and in varying degrees amenable to different research approaches that do not necessarily become obsolete. At the same time, the scope of any one approach is limited by the fact that the objects of political science are grounded in the beliefs and theories of political actors—including political scientists—and therefore historically variable, while the objects of natural science are in some sense “given” and independent of human discourse.

A second—and closely related—argument is that studies of the history of political science are necessarily to some degree

attempts to legitimate and delegitimize competing approaches. Although the authors lament what they take to be the distortions propagated by Whigs and skeptics, they insist that a historical account is always governed by the "perspective" of a particular identity and that "there can be no nonlegitimizing or neutral stance from which a disciplinary history can be written" (Dryzek and Leonard 1988, 1248). The authors, however, wish to make a virtue out of this apparent dilemma.

While philosopher-historians such as Thomas Kuhn have attacked many of the dogmas of empiricism and argued that the idea of a scientific method that could vouchsafe the progress of science is a myth, other historically minded philosophers such as Imre Lakatos and Alasdair MacIntyre (and we could add Larry Laudan and Stephen Toulmin) have not only accepted the unity of the history and philosophy of science but have attempted to ameliorate what they believe is the implicit relativism of Kuhn's position. They suggest that despite Kuhn's claims about the incommensurability of scientific paradigms, a certain transcontextual rationality of science can be discerned in its "historical development." The history of science can thus aid the philosophy of science in developing critical standards by demonstrating that one scientific paradigm or research program was better than another (at solving problems, explaining the success and failure of competitors, etc.).

Since natural science is hegemonic in its identity and history, an overall judgment of progress is possible. But in political science, where we have multiple and sometimes conflicting identities, history becomes particularly important for both orthodoxy and challenges to orthodoxy. If, however, these histories are to be more than self-serving and critical rhetorical exercises, as tends to be the case with respect to Whigs and skeptics, there must be some

sense of a standard of judgment that determines what is "better history."

These accounts, even though they are tied to particular identities, would "attend to episodes of political science in context . . . approached and judged for their success or failure according to how well they understood and resolved the problems *they* confronted" (Dryzek and Leonard 1988, 1257). Such histories would contribute to the "progress" of political science by (1) showing which methodologies are best for which context and (2) making the political scientist sensitive to the historical character of theories and the historical limits of a particular research program. The result would be progress measured in terms of a "'lateral' accumulation of politically useful research traditions, each of which is contextually constrained by its problem-solving power" (Dryzek and Leonard 1988, 1257).

My most immediate general reactions to this argument are both negative and positive. First, the somewhat strained neo-Mannheimian argument about how we are to derive unity out of plurality strikes me both as evidence of a lack of appreciation of the actual history of political science and as involving some significant confusions about the recent work in the history and philosophy of science. Second, I have a great deal of sympathy with the claim that we need—but have not achieved—adequate histories of political science that are in some significant sense truly historical in attitude and not simply part of a discourse of legitimation and critique. Furthermore, I agree with the general proposition that histories are, nevertheless, closely tied to issues of disciplinary identity and are an essential element in a critical self-consciousness about the field.

There are a number of problems with the exemplars the authors attempt to draw from the historiography of natural science. Not the least of these is the fact that they treat natural science as if it were

a unity comparable to the discipline of political science. The history of the natural sciences, as both activity and product, is considerably more diverse and complex than the authors suggest. There is no clear indication in this essay that the authors have explored the history of science much beyond the manner in which discussions of it have surfaced in the philosophy of science. But even if we approach natural science generically, there are difficulties with their analysis.

Unlike the case of political science, historians of natural science are not for the most part natural scientists but belong to a separate discipline whose product has very little directly to do either with influencing the discourse of science or with establishing the identity of the particular natural sciences. This is not to say that natural scientists have no sense of the history of their field or any concern about identity but only that these matters have little immediate relationship to formal historical accounts of science. Furthermore, the history of science is seldom governed in any significant way by issues in contemporary scientific discourses. What has traditionally informed the perspective of the history of science is a particular philosophy of science. For many years, such histories legitimated positivism; while more recent histories, such as those of Kuhn and Feyerabend, were designed to delegitimize positivism and support an alternative account of the logic and epistemology of science. And, like the history of science, the philosophy of science has been quite insulated from scientific practice.

The authors correctly note that during the last two decades or more (the post-positivist era) there has been a much closer relationship between the history and philosophy of science. Historians have been more philosophically sensitive, and philosophers have put more emphasis on the actual past and current practice of science. Some postpositivists, however,

have, as already noted, been concerned about what historicism implies and how merely attempting to understand science historically provides little in the way of critical purchase. They have sought to find rational standards for *philosophical* judgments about science that would account for what is assumed to be its cognitive or practical progress. But what must be emphasized is that although some of these individuals, such as Lakatos and other students of Karl Popper, may have hopes for passing on these standards to science, this whole discussion has had little to do with disciplinary identity and practice in the various sciences. There is a distinct failure in the article to distinguish between these philosophical arguments about the uses of history and the actual practice of science or to confront the issue of the relationship between them.

The authors tend to imply that the history of science and the kinds of historical-philosophical assessment discussed have become part of scientific practice and identity and that there is, given the historicity of political phenomena, reason to believe that they can and should play an even greater role in political science. The latter claim may be defensible but not on the basis of the putative analogy with natural science. Furthermore, just as the authors tend to reify the concept of natural science and equate its manifestation in the philosophy of science with scientific practice, they accept without analysis various and not always compatible philosophical claims associated with postempiricism and assume the applicability of such claims to political inquiry.

They draw somewhat indiscriminately on thinkers such as Kuhn, Lakatos, Hesse, Hacking, and Bhaskar, who are in many ways quite diverse; yet they offer summary conclusions that say, in effect, "Postempiricist philosophy of science has now shown. . . ." Some of the claims even seem to be strikingly out of phase with most of postempiricism, such as the idea

that the objects of natural science "exist quite independently of whatever the scientist may say about them." The weight of opinion (and even on occasion the claims of the authors) suggests that the objects of natural science have little status apart from their constitution in terms of the theories of science. It would seem more cogent to suggest that the objects of *social science*, constituted (as the authors insist) by the conventions and practices of actors, are quite independent of the claims of the political scientist.

Even as a philosophical claim (let alone as an assumption about what goes on in scientific practice) the argument about historical assessment of research programs is too complicated and contentious to be used as a wholesale authority for recommending how and why to do history in political science. One of the problems has been the regressive character of the argument that assumes theoretical assessment can be grounded in a consensus about problem solving or some other pragmatic criteria. In the process of transferring this argument to issues in political science the authors also slip into other difficulties.

In suggesting that approaches and research programs (Marxism, behavioralism, etc.) can be evaluated in terms of their contextual appropriateness for research, the authors tend to conflate theories and heuristic conceptual frameworks and to perpetuate a very dubious (and essentially positivist) instrumental theory of theories. Surely few serious Marxists (or behaviorists, DNA biologists, or the like) would settle for the notion that their claims about aspects of reality are merely useful and time-bound intellectual constructs. And this has nothing to do with the assumption of the historicity and conventionality of social phenomena, itself a theoretical claim and one on which (I assume) the authors confer a status greater than that of a contextually limited approach.

What distinguishes political science—and indeed, the social sciences in general—is that disciplinary discourse is not confined to substantive claims about its subject matter but is infused with epistemological and historical arguments that support competing identities, theories, and modes of inquiry. In the case of natural science, such arguments for the most part long ago "floated" up into, or became the basis of, the fields we now recognize as the history and philosophy of science. The authors correctly emphasize that these arguments in political science are tied to the lack of hegemony in the field, which also means that for the most part the history and philosophy of political science remain embedded in the disciplinary discourse rather than in separate metatheoretical enterprises. We may be able to strive for less-partisan histories, but it would take a considerable leap of fancy and faith to believe that more contextually sensitive histories will lead to a progressive resolution of cognitive dissidence and to consensus about rational criteria for judging research programs.

Dryzek and Leonard are not always sufficiently sensitive to the general character of the history that they urge should be more carefully and contextually examined. More "historical" histories might help us understand the internal dynamics of disciplinary evolution in the field and explain the pluralism and continuing crisis of identity. I very much sympathize with the belief that despite the plurality, we should seek histories that are less Whiggish and skeptical (even though I do not believe that these categories are quite adequate for discriminating past historiography); but I think that we must also ask, in a more historically reflective manner, why such histories have not been written. The problem goes far beyond competing identities.

Political scientists of all persuasions (and for various reasons) have tended to view and claim the history of political

thought as their history; and this in no small measure has inhibited looking at their real past. Other factors include the ahistorical influence of positivism, the liberal pragmatic ideology of progress, the racism and other embarrassing beliefs of our forebears, and even the boring unromantic character of much of the material.

It seems to me that even a cursory examination of the substance of the history of political science would indicate that it is becoming more dispersed rather than hegemonic but that at the same time this pluralism is leading less to the kind of productive conflict that the authors envision than to insulated enclaves. To understand this requires a grasp of the internal character of disciplinary evolution. Finally, even though there is no doubt that contextual sensitivity is important in writing good history, recent tendencies in historiography reflected in certain assumptions of the authors often equate contextualization with understanding. Even in the theory of biological evolution, environmental constraints are secondary to organic possibilities and accidents.

What bothers me, however, is that the histories the authors propose—oriented toward the dubious task of assessing research strategies in context—are likely to miss a great deal that is important about the history of political science. Although Seidelman's approach, for example, is very much informed by a critical purpose and a retrospectively imposed structure of analysis and may tell us little about the certain aspects of conceptual and theoretical evolution, it does tell us a great deal about the perceptions, purposes, and intentions of disciplinary actors and their strategies for dealing with such defining problematics as the relationship between political science and politics. Legitimizing research strategies within the discipline has probably on the whole been less important to political scientists than establishing their authority to speak truth to

power on the basis of a scientific identity.

Although political science may remain historically divided by its plural identities, I would urge an even more optimistic attitude than the authors' with respect to achieving nonlegitimizing, or neutral, history. Even without research on the history of political science passing to extra-disciplinary historians, it is hardly beyond the pale to seek histories that are less rhetorical than say, Ricci's, whose work is really designed not to explain the historical evolution of the field at all but rather to condemn what he takes to be its present character. The form and content are historical, but the argument is not. Even for purposes of critique, one's position might very well be better served by attempting to write more accurate and less factually vulnerable accounts. The truth might be as damning as fiction and even more interesting.

While we might agree with Dryzek and Leonard that histories are always written from a "perspective" and must start from some understanding of identity, it is important to discriminate between the sense in which this is obviously and necessarily true and the sense in which it is not. Historiographical assumptions, theoretical assumptions about the nature of historical phenomena, concepts governing problem definition and data selection, and the like are constitutive of the historical enterprise; but valorizing a particular identity need not be part of the game. Presentism is not a one-dimensional notion. History may be written to explain identities without judging or seeking to transform them. Unlike the behavioral era with its intense polarization within the field, the pluralistic present, for all its difficulties, may facilitate more nonpartisan history. It may be true, as Hayden White and others are quick to point out, that all history is plotted; but instead of the externally imposed plots, of which the authors are properly critical, we might attempt to elicit more immanent plots that are in-

trinsic to the internal development of the field.

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As but one author in a new, small, and doubtless expanding pond of disciplinary historians, I can only be grateful to Dryzek and Leonard for the able and critical attention my book receives in their recent article. They should be congratulated for bringing to the fore, in clear and elegant prose, many of the questions that need to be pondered by those concerned with the past, present, and future identities of our profession. I hope that they will succeed in opening up a much-needed and informed debate about who we are, whether we have a common history, and where, if anywhere, disciplinary denizens are going and should go, given our collective history and the recent interpretations of it.

While I do have a number of objections to the way Dryzek and Leonard criticize histories such as mine and how they discern what is important about disciplinary identity, I want to elucidate at the outset our important common ground, which others doubtlessly still find controversial.

In typical postempiricist fashion, they argue that methods and their findings do not have the same status in social science as they do in the natural sciences. For them this is a virtue as well as a necessity. It does not bring their version of the discipline crashing down. After all, we study human political actors and "socially constituted agents"; and we naturally and rightly have a plurality of identities that we hope to match to the changing world we study. Even if political science cannot provide the certainty promised by natural science, we have written and continue to write things worth knowing, even as the world and what is worth knowing changes. And we ourselves are part of the

political realities we study, bringing to them different concerns, traditions, and questions.

I agree with Dryzek and Leonard that political science has been and is, an increasingly large church reflective of its own diverse legacies and the realities we and our forebears have studied. Our contemporary common ground thus contains potentially rich legacies that are worth recovering and evaluating, as well as pockets of mud that we may as well try to ignore. The historical layers beneath our feet are often twisted and faulted—but they are just as often rich veins to those who would mine them for present uses. Dryzek and Leonard thus argue—rightly I think—that "political science is always rationally going to be home to a variety of research orientations. Any approach trying to demonstrate its utility or superiority must do so not just through reference to the extant range of traditions but also in relation to past traditions" (p. 1250).

This is all fine as far as I am concerned. No doubt others will want to take up their cudgels to defend the old-time scientific religion. Others will no doubt protest that Dryzek and Leonard have made suspect distinctions between the histories and practices of natural and social sciences. My objections lie elsewhere; for the moment we move beyond the venerable and unresolved debate about the scientific status of the discipline, all kinds of new controversies must be opened and old and forgotten ones reopened.

The issues really are, What is to be learned from our past? What is important about it, if anything? Does anyone care, besides political scientists? What ought we to care about, and how is it related to what our predecessors cared about? Who are the publics political science has tried to reach? Have political scientists contributed anything to the formation of the basic questions of political dialogue? Despite the diversity and variety of individuals who have called themselves politi-

cal scientists, is there anything that the best work and the most active profession builders had or have in common?

I am not sure that Dryzek and Leonard's way of analyzing current histories of the discipline provides answers to these queries. Their article positions them in moderate opposition to the simple categories of *Whig* and *skeptic*, to which their approach is supposed to be the necessary antidote. Those they label Whigs see the past as merely prologue, from which there is not much to be learned except how we got to the glorious present. Dryzek and Leonard are right to doubt whether we have reached political science heaven and whether everything before us was merely purgatory. Inversely, skeptical histories like my own are said to take Henry Ford's historical views equally seriously: we write history to show what bunk it really is. But come the revolution, . . . ?!

Not surprisingly, their placement of existing disciplinary histories into these dual categories is followed by their agenda for disciplinary history—one that is sensitive to “context,” plurality, and the multiple stories that follow from the “socially constituted and historically determined nature of the objects of political inquiry” (p. 1246). The past and present are made up of the good, the bad, and the ugly and always have been. But to Dryzek and Leonard, progress in political science can be defined by the “capacity to cope with contingency in the context of empirical problems.” We discuss our past not to surpass it but to test new approaches “through reference to the extant range of traditions but also in relation to past traditions” (p. 1250).

As one whom they place in the skeptical camp of disciplinary historians, I am not supposed to believe that there is much worth discussing here, since I have allegedly written political science history in order to destroy it. Current histories, they claim, are insensitive to contextual diversity. Like all such histories of the skeptical

camp, mine creates “hegemonic” traditions out of whole cloth, only to show later on that they are about to be superseded. Yet if intentions matter, my intention is not to create and destroy traditions in order to establish my own research agenda but to understand political science history as much as Dryzek and Leonard do.

I therefore find it surprising that their respect for the real diversity of real history is accompanied by a number of presentist assumptions that distort the intentions and knotty problems of our real history. While they want to reject the alleged presentism of current disciplinary histories, they only reflect presentist claims when they look at the discipline's history as simply a number of competing research traditions developed in specific political contexts. Perhaps Dryzek and Leonard rely too heavily on seeing the discipline from the inside—as a collection of various “research traditions” and scholarly debates with its own internal modes of verification, standards of judgment, and mode of establishing the legitimacy of its now historically sanctified and not-so-sanctified “traditions.” From Dryzek and Leonard's article, so respectful of history, I get the sense that our discipline is legitimated by its search for historically varying truths that are in turn ratified by how *we*, and *only we*, see them retrospectively.

Whatever the validity of this claim as a datum and a desideratum for political scientists today, I don't think this is what many of our forebears were doing or wanted to do, especially those who were important in creating the research traditions within which many of us still labor, albeit sometimes unconsciously. Leaving aside the fact that many of our forebears thought about science in the universalistic way critics now generally discard, many also thought about the discipline as verified by its way of posing and answering questions “in public”—in some sort of

public space that political science would help create, define, shape, and guide. Most of the time they saw the existing definitions of public life in the United States as wanting and provincial, a condition, however, remediable insofar as political science and other social sciences began to influence public dialogue by defining problems for the public agenda. In short, political scientists of the past did not just see the discipline as competing research traditions to be validated by intradisciplinary discussion among fellow professionals. Ultimately, political science was to be judged by its particular public consequences, in terms of the efficacy of its explanations and the authority it had achieved among the publics that political scientists wanted to influence.

I say "particular" public consequences because Dryzek and Leonard seem to argue that modern pleas for *relevance* have come only from leftists like myself or from those who contract their services to government, interest groups, or campaigns. Be that as it may, the church of relevance used to have quite different communicants and had doctrines about what kind of relevant salvation was to be sought through the science of politics. At least for the individuals Edward Harpham and I describe in *Disenchanted Realists*, political science relevance was certainly not something pursued solely either by leftists (in fact, they worried about that) or by those who sell technical services to the powers that be (they worried about that even more). *Relevance* is in fact not a word people like Woodrow Wilson or Charles Beard or Harold Lasswell would have used, for it is not just the applicability of any kind of research to any kind of outside political forces that interested them. They were much more interested in shaping and guiding the public agenda through the authority of the new discipline and its research products. That notorious radical skeptic Harold Lasswell perhaps said it best: "The hope of the pro-

fessors of social science, if not of the world, lies in the competitive strength of an elite based on vocabulary, footnotes, questionnaires and conditioned responses, against an elite based on vocabulary, poison gas, property and family prestige" (1935, 20).

According to Dryzek and Leonard, skeptical histories like my own try, but fail, to demonstrate that there is nothing worth salvaging in political science history. The political science greats were wrong about politics and about science from the start. On the contrary, my own history says they were right from the start about the same things, among which was their hope and awareness that the discipline was founded and operated for reasons beyond itself.

It would be downright stupid to claim that all or even most of the people who have called themselves political scientists have shared this perspective. My book, in fact, tries to tell the tale of the unintended consequences of the development of political science as a profession and a discipline. The research traditions about which Dryzek and Leonard speak began to have a life of their own quite apart from the purposes for which they were formed and from the public agenda individuals like Lasswell wanted to shape. Now, one may argue (as I do) that the language of science and the profession of political science were bad vehicles for shaping or even understanding the public agenda from the outset; but that is quite different from arguing that many of our research traditions were designed to be linked to U.S. political discourse in order to reform, shape, and change it.

It is important, then, that we see our history not just as a bunch of research traditions in a bunch of different historical contexts but through the eyes of those who had quite larger claims for it—claims whose vindication was the justification for the discipline's existence in the first place. Neither Harpham nor I made up

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the concerns of Lester Ward, Woodrow Wilson, Burnham, or Lowi, but we did give them a makeshift name to express this continuity of concern and (we argue) frustration. I might add that the figures we discuss are held together neither by the name we gave them nor a research tradition they have left us. Rather, some of our most prominent forebears possessed a common political sensibility from which many different kinds of methods and research traditions flow, whether we know it or not. What holds them together is a common sense that the language of U.S. politics is somehow premodern, that the eighteenth- and nineteenth-century practices, theories, and habits of the heart that sustained the U.S. republic are no longer adequate to the tasks faced by the U.S. polity. Various and even competing research traditions have been founded around this premise, and at different times the research traditions have found different maladies and proposed different solutions or despaired of solutions at all. Something bigger than a research tradition holds together scholars like Lester Ward and Theodore Lowi, Harold Lasswell and Arthur Bentley. Viewed from the inside of contemporary political science they have real and substantial differences. But viewed from a broader perspective of U.S. intellectual history, they and others have a remarkably similar view of the public, the state, and their troublesome relationships within the U.S. tradition. But despite the variety, what made political science worth starting and continuing were essentially variations on one theme, namely, the meaning of democracy in an epoch where the modern state had to be, and was being, formed. Political science of this ilk was born critical and partook in the broader intellectual climate that raised critical questions about the contemporary U.S. political order even as they raised hopes about the role of political science in answering these questions authoritatively.

While in my book I am sharply critical of the greats' implicit and explicit formulations of this problem and also disappointed about the sharp divergence between the intentions political scientists brought to their scholarly and public work and the disappointing results that have been achieved historically, I reserve the bulk of my ammunition for the contemporary discipline. Now, intentions seem somehow suspect; and professionalism too often means isolation in hermetically sealed subfields and languages of the Left, the Right, and those somewhere in between or nowhere at all. When political science matters nowadays, it seems to have lost its critical edge and its autonomous voice. Worse, it has lost the central core of questions and the common agenda that allowed others and ourselves to ask what was worth doing and why.

It may be symptomatic of what is wrong with our discipline that political science can be conceived of as nothing more than a jumble of research traditions from among which to choose. To write histories from such a perspective (as Dryzek and Leonard seem to suggest we ought to) itself neglects the richness of our own history. And at the risk of sounding anti-intellectual, it appears to me that the most meaningful part of our past consists not in retracing something like Charles Merriam's conceptualization of socialization but in finding out why he was interested in conceiving of it in the first place and why he himself became more than a little frustrated when people forgot to consider what all the research was supposed to be for.

The question of what political science is supposed to be for is not an invention, therefore, of the skeptical historians. It is a question that has been asked long before Ricci, Natchez, or other skeptics like me came along. Without asking the question, the discipline appears only as research traditions cleaved from their public effects, cleaved from publics and from its most

noble identities. It is true that our discipline has always been that way to some degree—but not to all degrees. And *that's* what made it, for many of our forebears, a noble science of politics. My judgment is not so much skeptical as ironic. We seem to have lost the conductors but have better instruments. And that's pretty much how Dryzek and Leonard see it today. They see it as good, I see it as bad. Whatever the case, the way we are now is not the way we were. It may be naughty or nice or neither; but it is notable, and I don't see Dryzek and Leonard noting it. This is the source of my unease.

Instead, what Dryzek and Leonard have given us are new internal standards and criterion by which to judge our own past by our own lights. That's fine but not enough. From inside the discipline, we see all the variety and complexity in our history, the fine distinctions in methods and findings that reveal differences. But from the outside, those who care to look might see a common thread once sewn through the variety and complexity of our historical fabric. Both perspectives are right; and both are needed, because only both do justice to our own history. To them and to us our past, present, and future are pluralist—and rightly so. But just as there are fine nuances to the initiated between Marx, Engels, Gramsci, and Lenin on the one hand and Gilder, Wanniski, Laffer, and Buchanan on the other, so we too have a certain unity in our diversity. Unity and diversity require different, although possibly complementary, criteria for the evaluation of any institution, including political science. To see both is true and needed. To see only one is partial and, in the case of our own profession, all too reassuring about our present status.

Perhaps one quote from Dryzek and Leonard is indicative of the partial truths that need to be made whole in the discipline and in our commentaries about it. They write, "As long as there is variety in orientations towards political science—

and this condition is to be fully expected as long as there is history and politics—there will be variety in disciplinary histories." Indeed. We have variety. But what kind of variety is it? Is it various enough historically to reflect or even to help form the boundaries of U.S. political discourse? For every important political tendency in the United States has there been a political scientist with a research tradition to represent it? I doubt it, just as I doubt Chevy, Ford, and Toyota automobiles represent the widest possible range of varieties in transportation, or Pepsi, Coke, and RC in liquid refreshment. We can disagree about whether our intellectual range is so bad, but it can't be presented as all there is politically. At the historical basis of our profession's existence is a definition of what constitutes a political position worth considering, a language worth speaking, and a public agenda worth our critical participation. While I am critical of our forebears in their particular formulation of these problems, I am even more disappointed when we ignore the questions they raise in our urge to do more research. The history of our discipline should, in some small way, prompt us to raise these questions again.

RAYMOND SEIDELMAN

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In our "History and Discipline in Political Science" (Dryzek and Leonard 1988) our intention was to explicate the role that disciplinary histories have and should play in shaping the identity of political science. We are pleased that James Farr, John Gunnell, and Raymond Seidelman—disciplinary historians of some repute—find our argument merits attention. We share some substantial points of agreement with them, including the idea that political scientists ought to be more attentive to the discipline's past and critically

self-conscious regarding the public, political implications of disciplinary discourse. But there are also points of disagreement, the most important of which turn on the implications of our different historiographical approaches for the shared concerns.

We develop these points by first noting several misrepresentations of our argument. Having cleared the ground for more productive dispute, we then argue that Farr, Gunnell, and perhaps even Seidelman hold problematic historiographical ideals that would consign disciplinary history to an intellectual ghetto, safe and separate from the contemporary practice of political science. We conclude by comparing and contrasting their approach with ours in terms of the public role and political responsibilities of political science and political scientists.

On reading our critics' accounts, we were initially struck by the different—even antithetical—conclusions they had drawn regarding the implications of our argument. For the most part Farr understands us perfectly, but we must protest his attempt to impress us into his platoon of *skeptical Whigs*. Seidelman accuses us of attending narrowly to the issue of choice across research traditions within the discipline to the exclusion of contemplation of the larger role of political science in public affairs. His accusation rests, we believe, on a mistaken rendering of our position as involving a kind of radical skepticism about the discipline. And Gunnell reads us as having conflated important metatheoretical distinctions between political science and the natural sciences, a Whiggish position absolutely contrary to that we do in fact hold.

Let us consider Farr first, since he most clearly expresses reservations about our distinction between disciplinary Whigs and skeptics (though Gunnell also registers, in passing, similar reservations). Farr is right—the distinction between the two schools is not absolute. Indeed, we spent

many pages showing that however much historians of political science may try to draw Whiggish or skeptical conclusions about the discipline, their own arguments support neither position. It may be useful, as Farr suggests, to discuss disciplinary histories in terms of other categories, although we would add that analytical frameworks ought to be judged by their usefulness in organizing the particular issues being addressed. Thus, Farr's (1988a) use of familiar methodological and political categories serves his purposes well; we found the Whigs-skeptics distinction illuminating for discussing issues of disciplinary progress.

In any case, what we find most suspect is Farr's attempt to situate us in his hybrid skeptical Whiggism. On the most abstract level, we share with Farr's skeptical Whigs a belief in progress and the need to respect the past by treating it as more than prologue to the present. But on Farr's own account, the histories of skeptical Whigs have no contemporary import, a position to which we do not subscribe. Hume and Smith wanted to write neutral, nonpartisan histories; and (however much they may have been sensitive to the possibility of *future* change) they really were Whigs in their belief in the superiority of *their* present. Hume's remark (cited by Farr) that Machiavelli was limited by having "lived in too early an age of the world" is to be taken in that sense. But Machiavelli did not live in "too early an age." Whatever greatness he achieved was in understanding his context clearly, not just anticipating what later Whigs would think important. At any rate, for such skeptical Whigs the past and the present never meet; and if we follow their historiographical lead, we must read their histories (and presumably write our own) guided by what would appear to be purely antiquarian interests. We, in contrast, posit an essential connection between history and identity, past and present.

Farr would have us conform to the

standards of Hume and Smith, exercising the "key virtues" of moderation, balance, respect, and tolerance. Of course, one might reasonably ask what is being moderated, balanced, respected and tolerated; and on these questions the distance between Farr's skeptical Whigs and ourselves is great indeed. We are concerned with modern political science, not "the romantic historicism of Tory or Jacobite reaction"; and the position we take certainly does not involve seeing that "Hume and Smith are right about the sort of detachment and moderation we can have as historians or political scientists." "Sympathy" for earlier times notwithstanding, we think there are good reasons not to buy into the historiographical self-understandings of the sage intellects of the Scottish Enlightenment. So while we might agree with Farr that skeptical Whigs are superior to skeptics or Whigs, their position is not without its difficulties; nor does it correspond to our own.

Seidelman, for his part, casts us not as skeptical Whigs but as the "moderate opposition" to contemporary Whigs and skeptics. In this, at least, Seidelman has located us in our context. But we must reject his implication that we attempt a balancing act that relies "too heavily on seeing the discipline from the inside," such that the past should be scrutinized only in terms of how it might inform present practitioners' choices across research traditions. This charge implies that we treat the discipline as nothing more than a holding company for traditions and ignore its actual or potential contributions to "some sort of public space that political science would help create, define, shape, and guide."

Seidelman implies that our disciplinary holding company is not a profit-maximizing concern, for it expects no return from its component traditions. He comes close to accusing us of a relativism in which "anything goes" in terms of traditions—which Seidelman believes captures the

current state of the discipline, as well as the essence of our argument. Seidelman may be right about the discipline, but he is wrong about our argument. Like him, we believe that studied indifference and mindless toleration comprise one of the major defects of contemporary political science, a point clearly embodied in our call for political scientists to recognize that productive argument within and across traditions and identities is essential, so that practitioners might better sort the wheat from the chaff (p. 1252). Indeed, one of our central messages is that such sorting is possible even if it is not obvious in the work of, nor recognized by, contemporary practitioners. And good disciplinary history, properly deployed, would help in this sorting.

As to Seidelman's claim that we ignore the discipline's contribution to political life, let us note that we stressed that political science scholarship should be judged by its performance in context (pp. 1255–57). This context and the problems it presents are defined not just by political scientists themselves but also by the larger society in which they move and that they help create (p. 1250). To be sure, the contribution of most political scientists—including Seidelman's greats—has been small; but they are only made smaller when compared to the inflated hopes of Seidelman's "disenchanted realists." Yet most of us are not greats, and we think there are good reasons why many would not want to be, at least if aspiring to greatness means contributing to public life in the way that many past figures wanted to. We can try to do our best in the situations in which we find ourselves, and we should recognize such efforts for what they are. It follows, then, that we should not only interpret the efforts of past figures in terms of their context but also—judge our own efforts, however meager they may be, in terms of their contribution to the resolution of contemporary social and political problems.

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Admittedly, *problem* is an ambiguous word. Problems can be conceptual and empirical—the grist for research in any endeavor, be it low-temperature physics or international relations. But problems can also be undesirable states of affairs: poverty, repression, antiquated institutions, sexism, racism, and the like. Seidelman accuses us of an almost exclusive concern with the former and a near indifference to the latter, concern for which would animate “critical questions about the contemporary U.S. political order.”

This charge comes as a surprise to us because, aside from occasionally writing about disciplinary history, we both consider ourselves critical theorists. Moreover, our most recent (separate) efforts in this regard are concerned, in different ways, with exactly the issue of how critical theory might speak to contemporary public life and how political science might change as a result (Dryzek 1987; it will also be taken up in Dryzek 1990, Leonard 1990; Leonard 1985). Seidelman might reply that none of this surfaces in our article. We would counter that at a minimum, we do not rule out *any* conception of “what political science is supposed to be for.” Any such disciplinary identity might be critical; but it might also be accommodating or technocratic. And as we argued, which of these identities political scientists accept should be in large part a function of the historical arguments adduced in their support. But here we can in fact go further and claim that a critical edge is at least implicit in our position. For the postempiricism we endorse is conducive to participatory democracy. Briefly (if crudely) put, by recognizing the historicity and contextuality of self-understandings and the need to subject those understandings to criticism from a wide variety of perspectives, a postempiricist worthy of the name would prefer to hear, rather than stifle, the voices of others. And given the relationship between social theory and political practice, participa-

tory and democratic politics would seem the best way to address the “critical questions” we collectively face.

Lack of space means that we cannot catalogue, still less respond to, all of Gunnell’s misinterpretations. Much of what he has to say will interest the professional disciplinary historian and epistemologist rather than the larger community of political scientists. So let us extract from his critique two particularly striking misattributions.

The first concerns our use of the history and philosophy of natural science. Gunnell claims we “tend to imply” that philosophical and historical discourse is central to the practice and identity of natural science and that we accept the authority of natural science to suggest that this centrality should apply to political science too. That is not our position. Indeed, we explicitly recognize, quoting Kuhn, that “science destroys its past” (p. 1249). Although as a matter of conceptual necessity disciplinary history should be central to natural science, the fact that in practice the natural sciences usually have hegemonic histories means that in most instances practitioners of the natural sciences can safely ignore historical issues. It is true that such historical issues and associated epistemological questions have indeed “floated up” (as Gunnell puts it) to separate disciplines—the history and philosophy of science. But we contend that our article explains exactly why this does not happen in political science and probably never will (see pp. 1249–52). (Let us note in passing that a more accurate characterization of our use of the history and philosophy of natural science may be found in Seidelman’s critique.)

Second, Gunnell seems to believe that our call for “contextually sensitive histories” implies a search for “consensus about rational criteria for judging research programs.” (Compare this charge of what might be called “metatheoretical Whiggism” with Seidelman’s claim that

we tender a kind of radical skepticism, with absolutely no criteria for such choices.) But our argument does not support—indeed, it is explicitly critical of—past attempts to (in Gunnell's words) "vouchsafe the progress of science" (see our claims at p. 1257). In fact, we believe that there are no universal rules, only particular arguments. This is precisely why political science needs disciplinary histories, namely in order to determine which kinds of arguments (including metatheoretical arguments) worked or failed, when they worked or failed, why they worked or failed, and which kinds of arguments *may* work or fail in the future.

Perhaps Gunnell might respond that our claims about the centrality of disciplinary history involve laying down some transhistorically applicable rules of rationality for judging research programs. If so, we would content that any such rules do not carry the epistemological status Gunnell seems to think they must. For us to suggest that there are good, rationally defensible grounds for choosing between theories and research programs is not necessarily to engage in the kind of metatheoretical nonsense of which Gunnell has been (rightly) critical throughout his distinguished career. Indeed, we would say it follows from our arguments that while postempiricism is the best metatheory *currently* available, it may later be replaced by a better alternative. If this should be Whiggism, it would be a very different Whiggism from that of which we (and Gunnell) have been critical.

Having suggested that our position is misrepresented when it is cast as skeptical Whiggism, radical skepticism or metatheoretical Whiggism, let us now turn to our critics' problematic historiographical claims.

The most glaring deficiency of our critics' arguments is their shared belief in the possibility of writing disciplinary histories that are more or less neutral with respect to disciplinary identity. As we

have already seen, this historiographical orientation is most pronounced for Farr, whose skeptical Whigs write avowedly neutral histories. Gunnell, too, believes that "less-partisan" and "nonlegitimizing, or neutral" histories are both possible and desirable. (He also, mistakenly, ascribes this belief to us.) And even Seidelman, that notorious skeptic and confessed critic of contemporary political science, endorses the neutralist position when he claims, "My intention is not to create and destroy traditions in order to establish my own research agenda but to understand political science history."

It is curious that in stating their commitment to neutralist disciplinary history, none of our three critics refers to the long passage in our article (pp. 1247–48), where we question this ideal, especially through reference to the failed efforts in this direction of Collini, Winch, and Burrow (1983). We have nothing to add to that argument, though we would note that it applies just as well to purported examples of neutral history such as Farr finds in Ward 1984. Many historians try to remain neutral, and many do not explicitly discuss identity issues; but it does not follow from the fact that people have tried to invent perpetual motion machines that they have, in fact, done so. There is a sense in which neutral histories (unlike perpetual motion machines) may be possible, but it is also this possibility that makes them undesirable.

One reason is that if disciplinary history cannot be brought to bear on contemporary debates, there is no reason at all for political scientists to attend to it. There is a deep contradiction here in the positions of all three of our critics. All three want disciplinary history to occupy a prominent place in the contemporary discipline—as Farr puts it, to "facilitate such conversations with past political scientists as take both their contexts and our concerns seriously." Yet all three commend a way of doing history that would

silence these conversations by rendering them pointless in terms of "our concerns." We can only be thankful that the published histories of Farr, Gunnell, and Siedelman fail to meet their own avowed neutralist standards and, as such, are worthwhile reading for contemporary practitioners.

But there is a second—and far more important—reason why neutral histories may be undesirable; for histories could be neutral only if there were no values (intellectual or political) to be contested. Such a world is possible, but it is not ours. In the real world, filled as it is with conflicts over values, a commitment to neutrality in writing history, be it of a discipline or a society, would serve only to cloak in the garb of nonpartisanship what is in reality a decidedly partisan position. Thus, purportedly neutralist history might flourish under a Stalin or a Deng Xiaoping; and even historians in the Western societies have not been exempt from a proclivity to reinforce or establish political consensus by writing so-called neutral histories. Such history might even flourish in intellectual disciplines where all "truths" have been finally settled. We have names for such practices, and they are not always approbations.

But the effects of dogmatism in politics and intellectual endeavors are familiar enough; we need not repeat them here. Suffice it to say that we find claims of—and calls for—neutral history intellectually indefensible and politically undesirable. This is why we made (in Gunnell's words) "a virtue out of [the] dilemma" of partisan histories and why we urged the cultivation of a reasoned disciplinary pluralism. But as we also said, reasoned pluralism does not imply indifference or mindless toleration (p. 1258). Bad arguments and shoddy histories should be exposed for what they are.

We firmly believe that this last comment embodies a commitment our critics share. Indeed, it is evident in their con-

cern that political scientists take their public, political responsibilities seriously. But we also think the historiographical ideals they advocate fit oddly with such concerns. Let us finally turn, then, to the question of public responsibilities, an important area of agreement with our critics.

All three of our critics are attentive to the need to address the "external," or extradisciplinary, public role of political science. Seidelman raises the issues in terms of political reform, Farr in terms of civic education. Gunnell says nothing directly relevant to these matters in his critique although, as we noted in our article (p. 1256), his previous work does embrace such concerns.

Admittedly, we said little about these issues in the text of our article except for one cryptic comment in its final sentence. But we were cognizant of them when we authored the essay (whence the cryptic comment). In fact, in prepublication drafts of the article Leonard twice inserted and Dryzek twice purged (only because he thought the essay had covered enough ground) a paragraph on the discipline's public role, here reproduced practically verbatim: "One point that we would like to emphasize is that the stakes in this [dispute about history and identity of political science] are not simply disciplinary—they are, at root, practical political stakes as well. Inasmuch as political scientists are also political actors and to the extent that what political scientists do has implications for shaping the attitudes, beliefs, and actions of other political actors, political science can contribute to the discourse of politics. And when political science is done well, political discourse stands to benefit."

It should now be clear that we would go further in our understanding of the discipline's public role than Farr and Seidelman (and perhaps Gunnell). They want to understand political reform and civic education as aspects of the intentions and ac-

complishments of past figures—as further grist, perhaps, for neutralist historians. Fair enough, but we would emphasize the need to understand these intentions in light of the *contemporary* public role and political responsibilities of the discipline. In this, contemporary practitioners might want to question whether past understandings of the public role of political scientists as, say, expert advisors to policy-making elites, defenders of liberal-democratic polities, and educators who teach citizens how to make the world safe for democracy (or democracy safe for the world?) are ideals we should embrace in light of the particular problems we face. And here, again, good disciplinary history can help.

By emphasizing that political scientists both shape, and are shaped by, the larger context in which they move and by arguing that good disciplinary history might better serve the discipline, we thought it would be understood that well-done disciplinary histories might also help political science better serve its public. But we believe that if the arguments of Farr, Gunnell, and Seidelman are taken to heart, disciplinary history will return to the intellectual ghetto from which our article tried to liberate it. This return would render political scientists less able to come to grips with the import of the public role they have played in the past and play in the present and less able to cultivate an adequate understanding of the role they should play in the future.

Good disciplinary history is, then, inextricably partisan both within and outside the hallowed halls of the academy. If a reader gleaned only one thing from our article, it should have been that partisanship is not the real problem facing political science or the writing of disciplinary history. The real problem is bad arguments. In fact, much of what our critics' have written elsewhere can be read as attacking bad arguments—bad arguments that have often gone by the name of nonpartisan

political science. We wish they had also recognized that an argument for nonpartisanship is no better even if only(?) disciplinary history is at issue. We won't push the point except to note that while we might all agree (with Santayana) that forgetting the past may mean we are condemned to fulfill it, we differ on what "remembering" the past would mean. To us, remembering the past involves more than simply the recollection of what our predecessors did and said. For what they did and said cannot be so easily distinguished from what we think and how we understand ourselves.

In all of this, the history of political science will be "partisan"—and partisan in a way directly relevant to contemporary concerns about the "internal" and "external" identity of the discipline. Our critics have done much—both intentionally and unintentionally—to show why this is the case. All of us are agreed that disciplinary history should be central to the discourse of practicing political scientists. But we clearly have some disagreements about the relationship between historiographical presuppositions and identity concerns within which this discourse should take shape. So we conclude as before, "What remains is to write histories that would sort out the lessons of the past in a way that future practitioners—and publics—might find useful."

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Forthcoming in September

The following articles, research notes, and controversies, have been tentatively scheduled for publication in the September 1990 issue:

David Austen-Smith and Jeffery Banks. "Stable Governments and the Allocation of Policy Portfolios."

Donna Bahry and Brian D. Silver. "Soviet Citizenship Participation on the Eve of Democratization."

Bruce Bueno de Mesquita and David Lalman. "Domestic Opposition to Foreign War."

Richard L. Hall and Frank Wayman. "Buying Time: Moneyed Interests and the Mobilization of Bias in Congressional Committees."

Ruth Lane. "Concrete Theory: An Emerging Political Method."

Michael Laver and Kenneth A. Shepsle. "Coalitions and Cabinet Government."

Leslie Paul Thiele. "The Agony of Politics: The Nietzschean Roots of Michel Foucault."

Kenneth Waltz. "Nuclear Myths and Political Realities."

John T. Williams. "The Political Manipulation of Macroeconomic Policy."

Ronald Wintrobe. "The Tinpot Totalitarian: A Simple Economic Theory of Dictatorship."

REVIEW ESSAY

THE CONTINUING SIGNIFICANCE OF RACE: THE TRANSFORMATION OF AMERICAN POLITICS

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Blacks and Social Change: Impact of the Civil Rights Movement in Southern Communities. By James W. Button. Princeton: Princeton University Press, 1989. 326p. \$29.95.

Issue Evolution: Race and the Transformation of American Politics. By Edward G. Carmines and James A. Stimson. Princeton: Princeton University Press, 1989. 271p. \$27.50.

The Struggle for Black Empowerment in New York City: Beyond the Politics of Pigmentation. By Charles Green and Basil Wilson. New York: Praeger, 1989. 183p. \$39.95.

The President and Civil Rights Policy: Leadership and Change. By Steven A. Shull. New York: Greenwood, 1989. 251p. \$42.95.

When academics break into print to proclaim the end of something, it is a likely sign that the phenomenon is about to reassert itself with a vengeance. The race issue has been one of those subjects.

In the late seventies and early eighties, some analysts said that as a result of progress in civil rights, race would no longer be a major issue in the United States. William Julius Wilson's book, *The Declining Significance of Race* (1980), contributed to the perception that the importance of race was decreasing and being eclipsed by class. The title received more attention than the argument about the improving position of the black middle class and the deteriorating position of the black underclass. Because overt discrimination against blacks was decreasing and the visibility of any one issue is short-lived, analysts too hastily concluded that race was no longer a major factor in U.S. politics. They conveniently ignored the possibility that class differences may

originate in racial considerations and that race might continue to be an important source of identity, a focus of political mobilization, and a powerful force in politics and public policy, just as ethnicity has been throughout U.S. history.

Four recently published books demonstrate that the importance of race is not declining. In an era when Jesse Jackson's presidential campaigns and black electoral successes including the victories of Dinkins in New York City and Wilder in Virginia have increased public awareness, they join a growing number of studies highlighting the continuing importance of race in U.S. politics. The four books underscore the persistent, pervasive impact of race and civil rights on U.S. institutions at both the national and local levels.

This common theme is particularly interesting because the authors are from different subfields within political science—elections, the presidency, local politics, and black politics—and didn't all assume

AMERICAN POLITICAL SCIENCE REVIEW
VOLUME 84 NO. 2 JUNE 1990

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the importance of race from the beginning. Carmines and Stimson started out to study national electoral processes and issue voting in general, not race issues per se. They "succumbed" to focusing on the evolution of race issues in elections because in the course of their work they recognized that race was transforming the U.S. political landscape in ways that other major events like recessions, world wars, and Vietnam had not. Shull's book is an outgrowth of his work on the president's role in public policy. He focused on civil rights because there were few comparative studies of how presidents from Eisenhower to Reagan had dealt with that issue. Button's previous work on black politics and the impact of black riots in the sixties led him to analyze black political mobilization in six small Southern cities in Florida and its effect on public services and private sector employment. Green and Wilson's book is also a study of local politics, particularly the strategies used by blacks to gain black empowerment in New York City.

Together, the four books constitute important evidence about the continuing impact of race not only in U.S. politics but also in political science. They help move the study of race, which has been marginal, closer to the center of the discipline. Just as Riker's book, *Federalism* (1964), showed that an understanding of our federal system required a recognition of its racial impact, these four books demonstrate that race has had a pervasive influence on many other U.S. institutions, an influence that accelerated in the 1960s and still continues, and that a full understanding of these institutions requires attention to race. Work on gender, race, and class is often criticized as "merely political" (a puzzling epithet coming from political scientists), rather than recognized as a genuine intellectual endeavor contributing to shaping the discipline. But these books support the claims that attention to race enriches traditional political analysis.

Both Button's book and the Carmines and Stimson book make major contributions to our understanding of the complex way the Civil Rights movement promoted change in U.S. political institutions. Both books reject mechanistic explanations in favor of more organic, interactive, evolutionary analyses of political and social change driven by electoral efforts over several decades.

Button capitalizes effectively on comparisons between three New Southern cities and three Old Southern cities in Florida over a thirty-year period to produce a valuable study of the evolution and impact of the Civil Rights movement at the community level. Button shows that the Civil Rights movement, when viewed from a local perspective, is complex and variable. Using a resource mobilization approach, he argues that the movement's local origins and the forms it took differed throughout the South and that its results have been quite mixed. Rather than being monolithic, the movement's developmental patterns and sequences varied greatly from community to community. There was no unilinear process of change, according to Button. Instead, the path of political development and the stages and rates of change were markedly different for each of the six communities.

Button argues that political culture is more important than the proportion of blacks in explaining variations in local responses to the Civil Rights movement. He shows that in the three New South cities black political mobilization started earlier than in the three Old South cities and involved independent bargaining and officeholding patterns. Electoral success was due to superior black political organization, black economic independence, middle-class black leaders, high black registration and turnout, relatively liberal white attitudes, and a fluid, open power structure. In contrast, in the three Old South cities, where these conditions were

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absent, blacks needed external support from the federal government and outside groups in order to achieve political change.

The most important result of the Civil Rights movement in Button's six cities was the election of blacks to local office. This political success produced significant changes in public sector services including police and fire, streets, and parks and recreation, with capital-intensive services showing more change than labor-intensive services and the New South cities showing more change than the Old South cities. However, black political success resulted in only moderate changes in the private sector despite the removal of blatant discrimination in employment and the integration of private sector institutions.

Carmines and Stimson also provide major new insights into race and the Civil Rights movement by analyzing the way the movement reverberated across, and permanently rearranged, the U.S. party system. Their National Science Foundation-funded 10-year study of a 50-year issue has produced an important book predicated on an interesting idea. They reject the realignment theory of critical elections as incomplete and propose an alternative theory of issue evolution. In their view, partisan change is not the result of a sudden and inevitable electoral shift but the result of an issue evolution that is dynamic, complex, subtle, and long-lasting. Their sophisticated analyses of time series roll call and electoral data show how the race issue gradually transformed the components of the U.S. political system.

In their theory of racial issue evolution, the rise of the new party politics was not the result of a single, realigning national presidential election but emerged from a long series of changes in presidential acts and party platforms, membership replacement and roll call votes in Congress, and changes in the attitudes and party identi-

cation of the mass electorate mediated by party activists. The unstable New Deal Democratic coalition composed of Southern conservatives, Northern liberals, and blacks was destroyed when race issues gained prominence. Internal contradictions were exacerbated by the external disruption of the Civil Rights movement and the emergence of a new ideology of racial conservatism in the Republican party that challenged the ideology of racial liberalism. Racial attitudes that once cut across party lines became polarized along partisan lines.

Decay of the New Deal coalition is a familiar theme, but Carmines and Stimson weave together a comprehensive and coherent argument about how the evolution of the race issue has produced a new division in the electorate. Analyzing four models of partisan change—impulse-decay, critical election, secular realignment, and dynamic growth—they favor the dynamic growth model that combines critical elections and secular realignment. A critical moment in 1963–64 involved massive partisan conversion and mobilization and set into motion a polarization process that increased over time via normal population replacement. In this theory elites do not primarily respond to changes in the mass electorate. Instead, elite leadership shapes issue conflicts and thus mass attitudes, and mass attitudes gradually shift toward existing elite positions. When the mass electorate grasps new positions and responds, issue evolution is underway.

Considering the same participants and time periods as Carmines and Stimson, Shull's *The President and Civil Rights Policy* presents a picture that is strangely different and static. The dynamic changes in partisan politics central to Carmines and Stimson's argument are missing. Instead after comparing presidential leadership efforts, Shull concludes that presidents are important in civil rights policy.

Focusing on seven presidents between

1953 and 1985, Shull compares the responses to presidential leadership by the legislature, bureaucracy, and judiciary in voting rights, school desegregation, equal employment, and fair housing. From a content analysis of presidential statements, an analysis of the *Congressional Quarterly* box score for legislative requests, and a review of administrative activities in seven agencies, Shull identifies a trend toward more presidential leadership in civil rights. Johnson and Reagan had the most influence on civil rights policy—Johnson primarily through legislative, Reagan through budgetary and administrative initiatives, according to Shull. He argues that Congress has provided the president less support in civil rights than in other policy areas but has still given presidents much of what they want and that in the 1970s and 1980s bureaucratic actions became more important than legislative actions and were less close to presidential preferences.

Although the tabulations of data and systematic comparisons of civil rights activities during seven presidencies may be useful to those who study the presidency and civil rights policies, the findings fail to get much beyond the familiar. It is hard to disagree with Shull's main argument that the president has substantial impact on the implementation of civil rights policy and that the stronger the ideological commitment, the stronger the impact. When Shull discusses Reagan's role, he simply repeats what we already know, namely, that Reagan reversed the direction of civil rights policy, changing it from a policy of expansion to one of contraction, reducing the enforcement of civil rights.

Shull's failure to uncover the dramatic partisan shifts analyzed in such depth by Carmines and Stimson is a major gap in his treatment of civil rights policy. Shull simply says that congressional voting on civil rights has not historically been very partisan or conflictual but became more

so in the 1970s. According to Shull, party pronouncements have influenced agenda setting, but the parties play lesser roles than interest groups in the later stages of the policy cycle.

The book on black politics in New York City by Green and Wilson lacks a coherent argument. Thus, it doesn't give us as much new insight into black politics as it might have. The topics covered need to be analyzed in more depth and linked to more of the relevant literature on New York City politics.

Reviewing the historical exclusion of blacks from positions of power and the negative impact of this on black civil society, they identify three periods: Irish hegemony from 1880 to 1932, white ethnic symmetry from 1933 to 1976, and white backlash from the 1977 election of Mayor Koch. In the post-civil rights era, a new, more fragmented black politics has emerged using electoral rather than protest strategies. There are splits between black elected officials who are party loyalists, those who are more committed to black empowerment, and other black activists outside the electoral arena. In the authors' view, the decline of political clubs and the growing political activism of black churches has increased the power of black churches and the fragmentation of the black community. Examining the splits between African-American and African-Caribbean groups, they urge the formation of a broad umbrella of pan-Africanism. They also call for a progressive coalition for black empowerment capable of moving beyond backlash but acknowledge that the obstacles to such a coalition are greater in New York than in Cleveland, New Orleans, Chicago, and Boston because of New York's ethnic pluralism and borough structure. Nevertheless, they argue—and events have proven them to be correct—that the coalition promoted by the Jesse Jackson campaign can be translated into the election of a black mayor.

The Significance of Race

What are the major contributions of these books to political science? First, they reassert the importance of race as a subject for political analysis. It is at the heart of significant changes taking place in both Southern and Northern local politics and in national political institutions and partisan alignments. Race has been, and continues to be, central to an understanding of the workings of our political system.

Second, the Button book and the Carmines and Stimson book both develop explanations of political change. In doing so, they demonstrate the limitations of the cross-sectional and narrow-gauge perspective of much of traditional behavioral political science. By expanding their time frame and focus and by using a broader array of data and methods, the two studies reveal developments that would have been lost in narrower, more mechanistic treatments. What stands out is the variability of political change in different communities and the cumulation of gradual changes over time in national partisan politics as a result of race issues. Yet these racial impacts might have been missed if the only criterion was influence on immediate behavior. Carmines and Stimson put it well when they say that "we have an excessive tendency to concentrate on the here and now, a blindness toward movements on a grander time scale" (p. 197). So these studies of race alert us to processes of political change and to methods of studying those changes that produce more complex, sophisticated, and comprehensive theories of politics.

Third, all of the books highlight the importance of leadership and all except the Shull book also emphasize electoral coalitions and strategies, themes that are most applicable to other issues and groups. In the Southern cities black elected officials and electoral strategies involving broad coalitions were key to political change. Protest strategies complemented these traditional strategies in a pattern similar to

the one that Rufus Browning, David Tabb, and I identified in California cities (1984). Individual initiatives and broad coalitions are shown to be essential to black empowerment, involving a sharing, not a monopoly, of power.

The most striking limitation of these books is that they give only the briefest attention to questions about the future and the generalizability of their findings. How likely is it that the trends identified in these books are going to continue? How generalizable are their findings to other issues, times, and contexts? What lessons would the authors draw from their historical analyses about strategies for racial equality in the coming decade? Button's resource mobilization theory of social movements leads him to conclude that while permanent progress has been made, conditions now are not conducive to dramatic new phases in the Civil Rights movement. Carmines and Stimson say that issue evolutions seem inevitable after the fact but are almost unknowable before it, so they do not even speculate how long it will be before the current alignment decays or how it relates to other potential partisan alignments on economic or foreign policy issues. Green and Wilson are the most explicit about the future, saying that when the white United States acclimates itself to having black people in positions of power, it will be possible to move beyond the politics of pigmentation.

Given the rapidly changing racial demographics and the political-economic shifts already underway, it seems realistic to predict that splits within and among racial groups will accelerate, producing a mosaic of stresses and strains. Continuing change in the nature of racial issues seems quite likely. An end to the relevance of race seems entirely improbable. As the four books reviewed here make clear, past predictions about the declining significance of race have been grossly inaccurate and misleading. These books open our eyes to the pervasive ways race issues

manifest themselves and help correct the gaps in our models. We must avoid being fooled by the ebb and flow of public attention into proclaiming the end of the

race issue when we should be acknowledging that it continues to shape and transform U.S. politics.

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POLITICAL THEORY

**Self-Direction and Political Legitimacy:
Rousseau and Herder.** By F. M. Barnard.
Oxford: Clarendon, 1988. 330p. \$69.00.

This book compares the political thought of Rousseau and Herder in an attempt to determine whether, as some have claimed, Herder was a German Rousseau. Barnard gives a qualified answer.

There is much that the two thinkers held in common. Each is viewed as evoking the tradition of civic humanism against the destructive dimensions of modern culture. The latter makes people into self-centered, rootless persons who are incapable of thinking as citizens devoted to the common good and to the nation. Herder's "extended self," a concept of self held by Rousseau as well, resists the destructive aspects of modern culture and is made the basis for authentic human identity. The extended self always exists not in isolation from, but in relation to, other selves; and this experience is the basis for discovering "the meaning of a common interest" (p. 7). To define the common interest, the extended self must mediate or accommodate the differences among people through the process of discursive thinking. The latter, which establishes reasonable grounds for public life that all can accept, in addition permits citizens to be the authors of their own actions, governed by a sense of their own autonomy and freed from dependence upon any external master (p. 288).

The basis for a mutuality, however, may not always be discursive thinking alone. "Reasons of the heart or non-deliberate dispositions were to both of them far more potent springs of human conduct than discursive reasons of the intellect" (p. 289). For both, religion, especially when located within the particular cultural setting of the nation, becomes a powerful source of civic-mindedness and national commitment. Each emphasized the teaching of a moral disposition to identify with the nation, whether through public ceremonies (Rousseau) or through ethnicity, folk-songs, myths, and legends (Herder). For Herder and Rousseau, then, "a culture of civic humanism

is as much a matrix of dispositions and intrinsic feelings as it is the work of deliberative reason and external agreement" (p. 217).

In constructing this perspective, each thinker holds different views of the best way to approach diversity. For Rousseau, nations are founded by a godlike legislator or lawgiver, who molds the people into a nation by instilling a common political culture of customs and habits of thinking and feeling. By contrast, Herder says people are born into a particular culture with a common language, and as these intersubjective experiences "unfold" and "grow," people mature within them as self-directing agents and as individuals who think of themselves as members of a particular nation. Herder's account of culture places an "inestimable value" upon diversity, whereas Rousseau sees diversity as a threat to unity. While politics for Rousseau must "dissolve diversity *into* unity," for Herder, politics, influenced by culture's unfolding, must "preserve diversity *amidst* unity" (p. 307).

For Barnard, Herder claims that even if the constitution is located in the broader political culture, manifested by the nondeliberate dispositions embedded in language, the constitution is a "causality in its own right" and thus must "fashion" and "shape" the "broader political culture out of which it emerges" (p. 239). The mechanisms and actions of government would have responsibility for protecting the diversity that the culture nurtures. But it is quite possible that culture can signify *such* a strong feeling for the nation (at the same time as it generates respect for diversity) that citizens become willing to sacrifice to the needs of the nation some of the political freedoms that protect diversity. Indeed, Barnard's Herder says that a nation may lose its state and be deprived of its political freedom; but if it is "not robbed of its culture, and if it upholds its language, it will survive as a place its members feel at home in" (p. 238). In this case, culture might replace liberal sentiments with extreme nationalistic ones, irrespective of a constitutional intent to do otherwise. And then Herder's state would treat diversity the same

way that Rousseau's general will did.

The civic humanist perspective of J. G. A. Pocock's *The Machiavellian Moment* (1975) views the state as a self-governing republic composed of active citizens who accept the restraints deemed necessary to secure the common good. In Judith Shklar's study of Rousseau, *Men and Citizens* (1969), Rousseau's republic is maintained by an austere adherence to externally imposed norms. Here, the civic perspective may work to undermine respect for rights and individual choice. Other writers hold up the model of a liberal republic that seeks to cultivate civic sentiments but without undermining both rights and respect for individual choice. Thomas Pangle's *Montesquieu's Philosophy of Liberalism* (1973) and David Epstein's *The Political Theory of "The Federalist"* (1984) are examples of this kind of work. Barnard's book adds admirably to these themes in scholarship; and in contrasting Rousseau to Herder, Barnard can be viewed as providing a richly textured depiction of the civic culture of a liberal republic.

STEVEN M. DELUE

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Democracy and the Contemporary State. By Frank Bealey. New York: Oxford University Press, 1988. 285p. \$57.00.

Frank Bealey combines a discussion of democracy as an abstract concept with an attempt to describe how democracy actually functions in the contemporary world. He contends that "it is of little significance to consider the operation of democracy separately from the exercise of power and that it shows a misunderstanding of democratic processes to describe their outcomes as 'undemocratic' merely when we find them unfair, or unjust, or distasteful."

Bealey argues that democracy is not necessarily more committed to equality than other political systems except in its guarantees of citizenship and equality before the law. There are autocrats who treat everyone the same, and "we have numerous examples of democratic countries where the majority of the electorate seem to have opted for greater inequality" (p. 130). Democracy does not guarantee a

good, fair, or just society (p. 8). What constitutes a good, just, or fair society is a matter of opinion; and all that democracy does is to make room for debate. Democracy depends on commitment to certain virtues, such as tolerance. Trust is even more important than tolerance because it relates to our expectations of other people's behavior. We trust our opponents to honor our democratic freedoms and to respect democratic procedures.

According to Bealey, there are two dangers in the claim that democracy is the most stable form of government: that we will subordinate democracy to the value of stability and that we will not see that democracy may actually encourage instability because it allows people to engage in public disputes: "The obsession of the 'consensualists' with stability misses the point. What endangers democracy is not the propagation of radical ends (or, for that matter, reactionary ends) but the pursuit of any ends by methods that break the law or the constitutional rules and conventions" (p. 26).

While Bealey thinks that the question of justice and the good are matters of opinion, he is quite confident that we can know what democracy means. "Democracy, a political regime found in some modern states, is a decision-making process which can exist when public contestation, inclusiveness, and responsible government are present; and which can only be maintained when there is a rule of law" (p. 28). But the existence of a consensus in a large part of the world concerning the meaning of democracy does not make the term *democracy* less a matter of opinion than other value concepts. Bealey regards beliefs about justice and the good society to be matters of opinion because he collapses the distinction between moral reasoning and interest group bargaining (p. 132).

Readers interested in the links between political philosophy and the contemporary political scene should learn a great deal from Bealey's book.

LYLE DOWNING

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Book Reviews: Political Theory

Social Feminism. By Naomi Black. Ithaca: Cornell University Press, 1989. 390p. \$47.50 cloth, \$12.95 paper.

This book represents a bold challenge to both the theoretical and practical definitions of feminism current today. Black rejects the accepted categories of feminism because, she claims, she can find no place for her beliefs in these categories. Instead, she divides feminism into two opposed categories: equity feminism and social feminism. Equity feminists want to make women equal to men; social feminists, on the other hand, want to emphasize women's difference from men, valorize that difference, and derive a public role for women from their private role. This categorization alone sets Black's book apart from many contemporary feminist works. But another aspect of her work is also unusual: she defines social feminism through the analysis of feminist organizations. Arguing that the reality of feminism is ignored by many definitions, Black proposes to illustrate social feminism through the examination of three organizations that embody social feminist beliefs: the Women's Cooperative Guild in England, the French Union Feminine Civique et Sociale, and the United States' League of Women Voters.

The themes that Black develops in her analysis of these organizations reveal her understanding of social feminism. The women in these organizations seek empowerment and autonomy, but they do so through the specificity of their place as women rather than through claims of equity. Central to Black's definition of social feminism is women's role in the family. This role motivates women to provide social services to an extended family—society as a whole. It is this perspective that defines social feminism's impetus for social reform. "Social feminism," she argues, "made it possible for women to add a political role without disrupting their self-image or the expectations of the men around them" (p. 61). In her conclusion Black stresses that it is the social feminist emphasis on women's difference that offers the most promising basis for radical social change. She argues that social feminism provides a rationale for women to act differently in politics, to reject its more "characteristically masculine and undesirable aspects" (p. 243). Although she admits that social feminism entails certain organizational risks

(particularly the threat of maternal dominance), she nevertheless argues that the practical activism generated by social feminism is the most powerful position available to feminism today.

Black is frequently defensive in her advocacy of social feminism, noting that this position is regarded by many feminists as conservative, even reactionary, and only in a marginal sense feminist. In her discussion of the three organizations she analyzes, Black takes great pains to present what may seem to be conservative positions in a more positive light. In the case of each of the organizations her thesis is that an initially conservative position was transformed into a more progressive stance and that the impetus for change was social feminist values. The historical record she cites, however, fails to establish her position firmly. Although the organizations she studies mobilized traditional women, they neither advocated nor achieved radical social change; and in several cases they advocated conservative causes or abandoned the cause of feminism altogether.

Other problems are also evident in Black's argument. Although she claims that her intent is to look to feminist practice rather than feminist theory, her avoidance of theoretical analysis hampers the development of her argument. She makes no distinction between arguments of women's difference based on biology as opposed to those based on social construction. As a result, she classifies feminists such as Alice Rossi, who advocates a controversial, biologically based theory of difference, with a theorist like Catherine MacKinnon, who claims that it is the socially constructed categories of the feminine that disempower women. Placing these two theorists in the same category requires substantial justification that Black fails to provide. To confuse matters further, Black blurs the distinction between her own two categories. In her analysis of the League of Women Voters she claims that the values of social and equity feminists tended to merge in practice: "But of course social feminists have always wanted equality as well as a transformed environment" (p. 274).

A more fundamental theoretical problem, however, distorts Black's entire project. Her argument revolves around a controversial advocacy of difference. But the notion of difference entails a standard that defines vari-

ance—a standard that can only be masculine. Thus, in a theory based on difference the masculine is always the standard of judgment. This is a serious obstacle to her argument. A related problem, however, lies in her attempt to define “women’s perspective” as the locus of that difference. Although she admits at the outset that her examples are exclusively from white, middle-class, European and North American women, this qualifier is abandoned in the subsequent analysis. The “women’s perspective” she seeks to define as the basis of social feminism is only the perspective of the class and race of women that she has chosen to study. Thus, the perspective of “difference” from which women are to launch their transformation of politics is not, as she claims, universal but race- and class-specific. These theoretical problems cannot be ignored. Although Black looks to political practice for her definition of feminism, she overlooks the very issue that, she claims, informs that analysis: the historical and cultural specificity of women’s place in society. Such a perspective cannot be defined, as she seeks to, in the singular but only in terms of the particular circumstances of specific groups of women.

SUSAN HEKMAN

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The Resurrection of Nature: Political Theory and the Human Character. By J. Budziszewski. Ithaca: Cornell University Press, 1986. 218p. \$22.50.

The Nearest Coast of Darkness: A Vindication of the Politics of Virtue. By J. Budziszewski. Ithaca: Cornell University Press, 1988. 201p. \$24.95.

These two works defend a naturalistic justification of the politics of virtues from a religious point of view. Until recently, the concern for virtue among theorists in departments of political science was centered almost entirely in the Straussian school, which upheld the secularism of classical natural right theory and opposed a rapprochement with Biblical theology. Recently, Alasdair MacIntyre’s *After Virtue* has broken the Straussian monopoly on this subject. Like the Chicagoans, MacIntyre is inspired by Aristotle and largely devoid of theological concerns. But

unlike the Straussians, whose position is articulated in Harry Jaffa’s *Thomism and Aristotelianism*, MacIntyre’s account is confessedly conventionalist while at the same time vaguely regretful that his unfinished work does not deal with theological issues.

Budziszewski here steps in to fill this theological gap. Fully aware of the secular inclinations of his academic readers, however, our author chooses the path of the soft sell. Thus, the earlier book sets out to defend ethical naturalism on grounds of most importance to secularists (especially liberal secularists) and Christians alike: Is the diversity of human culture and of individual aims expressed and underscored by liberal principles compatible with a standard of excellence based on any coherent notion of human nature?

Rejecting absolutist or perfectionist accounts of virtue, the author argues that liberalism is compatible with a “Lapsarian” version of the politics of virtues in which the good judgment that entails virtue must be brought into play. Such a view is necessarily opposed to most liberal procedures, whether of Locke, the utilitarians, or Rawls. In categorizing the best state, their beginning procedures ignore, or preclude the existence of, the kind of excellence in individuals that Aristotle requires. In Rawls, as well as in the utilitarians, we have at best a standard of civic excellence that comes at the end of a long procedure, a procedure that ignores the importance of individual character at the beginning.

Now, this criticism is a bit strong, since Rawls at least presumes a rational and disinterested inhabitant of the original position. But while the procedures and assumptions of liberalism may be flawed, Budziszewski nonetheless argues that under certain circumstances the liberal principles of equal rights and liberties are not only compatible with his view of the politics of virtues but in some degree are entailed by it. Even if excellence of character was the only distributive criterion, no greater inequality could be justified under it than under most liberal principles. Thus, if true poverty prevents one from exercising one’s own excellence, virtue cannot be used to justify continued oppression. Furthermore, immutable ascriptive criteria are no more incompatible with the politics of virtue than with the liberalism of Locke, for whom such criteria require only the consent of scoundrels.

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Budziszewski admits that liberals like Mill and Tocqueville do have a notion of human excellence and thus have some regard for the relationship between private virtue and public worth. Yet the plurality of ends and means in liberalism falls far short of Aristotelian standards for discerning the best state. Rather cautiously, Budziszewski argues that the very tendency of Mill's liberalism to obscure the unity of aims makes it problematical that a republic will always be at the apex of liberal choices. His politics of virtues would continue the unified elegance of the Aristotelian taxonomy that would guarantee those just and generous institutions more ably than liberalism itself can.

In the second book Budziszewski addresses the problem of Christian polity more directly. As a book, it is less coherent than the first. It consists of five discrete essays on varying topics tied together by the view that none of the modern secular political theories or methods can satisfy the claims to excellence. If the first book attempted to show certain lines of compatibility between liberal ends and the ends of a politics of virtue, the second asserts the incompatibility of the politics of virtue with elitism or paternalism—with the so-called conservatism of free market liberalism or that of Oakeshott. For Budziszewski, the only acceptable conservatism is that of St. Thomas because it extends beyond the "speechless reason" of classical Burkeianism and is itself compatible with certain liberalisms; for if religion is a mainstay of the politics of virtue, so must reason be also.

But the reason we need is not found in the causal explanations of modern political scientists but in the understanding of a Tocqueville who looks into the souls of subjects. It is a reasoning that is employed to look at the democratic citizen to discover whether he is committed to freedom or to the "resentment" of totalitarian democracy. Understanding is fulfilled by what we might call a narrative Christianity, one that replaces Socrates' pagan life story of "learning how to die" with the Christian one of learning how to live.

Not all of this is equally strong. In both books, the arguments are sometimes elegantly economical and their specificity sometimes suffers accordingly. It is not easy to overturn Oakeshott in 10-or-so pages, and we don't really have to accept his argument that liberals

would regard their principles of equal rights as absurd because taken to extremes they would give us rights to other peoples' children. The author himself has stressed the private, hence self-limiting, nature of liberal rights.

But there are some very good things here; while I am not won over to the author's confessedly shy defense of a specifically Christian politics of virtues, Budziszewski's juxtaposition of naturalistic virtue with competing philosophical traditions is bold and convincing. Both books are well worth reading (especially the first) and should be read in order.

JOHN L. STANLEY

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Democracy and Its Critics. By Robert A. Dahl.
New Haven: Yale University Press, 1989.
397p. \$29.95.

When Robert Dahl speaks about democracy, everyone should listen. With *Democracy and Its Critics* Dahl has produced a work destined to become another classic to go along with his *A Preface to Democratic Theory*, which won for him the 1989 Lippincott Award for a work of exceptional quality by a living political theorist that is still considered significant 15 years after publication. Indeed, it might just be prudent for the council of the American Political Science Association promptly to decree that it is permissible for the same person to receive the Lippincott two times so there will be no confusion 15 years from now.

A Preface to Democratic Theory (1956) was a model of crystalline elegance—a work of lean, disciplined reasoning, with hardly a superfluous word. *Who Governs?* (1961) gave more than a hint as to where Dahl's theory of democracy was taking him, which then came together magnificently in *Poliarchy* (1971), a work that with explosive force opened our minds to novel ways of thinking about pluralistic democracy, a topic that commanded reverence but had seemingly become too old-shoe to demand rigorous logic. Now with *Democracy and Its Critics* Dahl's virtuosity takes the form of conducting a masterful dialogue of reason, of guiding a truly ad-

vanced seminar on the major ideas and arguments in the field of democratic theory broadly defined. The reader becomes a mute but engrossed participant in a brilliant but also meticulously detailed series of discussions in which few issues are skipped over or arguments left hanging, as almost every train of thought is carried to its logical termination.

The atmosphere of a sustained seminar is reinforced by Dahl's ingenious use of the playwright's technique of heated dialogues and verbal crossing of swords. Thus, Aristros clashes with Demo on questions of guardianship; Majoritarian takes on Critic over majority rule and democratic processes; Advocate and Critic explore a variety of issues about procedural and substantive justice; and we have Modernist versus Traditionalist on questions about the common good. On federalism and the majority principle there is Jean-Jacques, whom we assume to be Rousseau, versus James, who must be Madison; but then Dahl playfully startles us by having Jean-Jacques cite Rousseau in his support.

Although the tone is always disinterested objectivity, the work is driven by an intense passion; but it is hard to tell whether the passion is more Dahl's commitment to democracy or his attachment to reasonableness. For Dahl the ideal is unqualified democracy, but at the same time every argument has to bow to the sovereignty of reasonableness.

The overarching historical thrust of his analysis is the progression of three major transformations of democracy. First, there was the birth of democracy in the city-state dimensions of Pericles' Athens, the Roman republic, and Renaissance Italy. The second transformation involved the emergence of large-scale representative democracy from the eighteenth through the twentieth century. Finally, Dahl raises the question whether we may be now experiencing a third democratic transformation that will involve the spreading of democracy, especially in the form of greatly increased participation in not just politics but also economic activities.

Although Dahl is contagiously enthusiastic about the prospects for the third transformation, his vision is somewhat puzzling. At a time when the great drama in world affairs is precisely the spectacle of all kinds of transitions toward democracy, he remains surprisingly skeptical about the prospects for democ-

racy to spread beyond the club of the long-established democracies. Instead, he reserves his excitement for ideas popular in the sixties and seventies about the spread of democracy within U.S. society and our domestic institutions. The possibilities range from the better incorporation of minorities and women into politics to the need for greater income equality and other issues that have been high on the agenda of the liberal wing of the Democratic party.

While one can respect Dahl's personal preferences that have focused his vision, the failure to address more earnestly the question of democratic transformations in a comparative context is troubling on several scores. First, we know from his *Poliarchy* that Dahl has given serious thought to the conditions favorable for the development of poliarchy, but in this book he slights that important subject. He does acknowledge that beliefs, attitudes, and political culture are important for democracy; but he engages in little analysis of these subjects, referring us only to Almond and Verba's *Civic Culture*. At this time in history we need Robert Dahl's wisdom about the specific cultural characteristics that can either facilitate or retard the development of democracy.

Secondly, by not analyzing the problems of democracy in a comparative context more thoroughly, Dahl seems somewhat insensitive to the importance of institutional arrangements for the fate of democracy. Why is it, for example, that except in the United States presidential systems have all too frequently reverted to periods of authoritarian or military rule, while cabinet systems with prime ministers have been better at preserving democracy? Also, isn't it significant that no other country has chosen to copy the extreme form of checks and balances of the U.S. system? Why is it that U.S. citizens alone rejoice over having a system of government in which not only do the executive and legislature obstruct each other but departments and committees within each branch contribute to ensuring that no policy can be coherently designed and implemented? We end up being governed by unintended consequences. What is it that sustains the U.S. faith that rational public policies are possible when we know perfectly well that by the time the policy-making process is completed, the result will be

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a mishmash of compromises? Could the answer be that U.S. citizens in general share Robert Dahl's profound faith that democracy and reasonableness are indistinguishable and that therefore if the process is democratic, the outcome should not be far from being reasonable?

Robert Dahl has done so much to expand our understanding of democratic theory that maybe it is unreasonable to ask that he examine empirically the prospects of democratic transformations in political cultures other than the United States and the well-established Western democracies. The request is not unfair, for Dahl explicitly insists that he is working in both normative and empirical theory. His empirical world, however, is largely of the realities described in the literature of Plato, Aristotle, Locke, Rousseau, Pareto, Mosca, Montesquieu, Tocqueville, Sartori, and numerous other worthies. It is a rich reality, and Dahl is completely at home in it; but the contemporary world is also producing exciting but puzzling realities that need to be confronted if there is to be an expansion of our knowledge about democracy. Let us therefore hope that the comparative transformation of democracy is high on Robert Dahl's research agenda.

LUCIAN W. PYE

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Machiavelli and Mystery of State. By Peter S. Donaldson. New York: Cambridge University Press, 1988. 227p. \$32.50.

The title of this book creates an almost Machiavellian illusion, suggesting that the project at hand is one thing when in fact it is something quite different. Peter Donaldson does not actually explore the relationship between Machiavelli and "an ancient tradition of secret political instruction" (p. vii) called the mystery of state because, historically, no such connection actually exists. Instead, Donaldson offers "a study of six figures—five writers of political texts [Cardinal Reginald Pole, Bishop Stephen Gardiner, Arnold Clapmar, Gabriel Naudé and Louis Machon] and one printer of political literature [John Wolfe] for whom, in various ways, Machiavelli was associated with

ancient and secret knowledge, with mysteries of state or *arcana imperii*" (p. viii).

Donaldson does not formally advance a contestable thesis. Rather, he seeks to make "a contribution to the history of Renaissance political discourse" (p. xii) by showing how "Machiavelli's ideas were received, transformed, and restated" by six thinkers who linked them with "the myth of the *arcana imperii*" and by indicating how this particular perspective on the great Florentine's thought produced "changes in the meanings of what it was to write and publish a political text, changes in how authors of such texts thought of themselves, their activity, and their audience" (p. 86).

What Donaldson does most successfully is reveal how Machiavelli's innovative, iconoclastic, and paradoxical political theory was given widely divergent interpretations and moral evaluations by a group of Renaissance thinkers who all viewed his texts "as if they were a kind of secret manual or brevarium" (p. xii). For Cardinal Pole (chap. 1), "the first major opponent of Machiavelli" (p. 30), *Il Principe* was a secret, demonic text that revealed the *mysterium iniquitatis* (Thessalonians), provided the covert ideology of the Henrician state, and prefigured the coming of the Antichrist. For Bishop Gardiner (chap. 2), in contrast, Machiavelli's works "were repositories of the so-called *prisca theologia*, or ancient hermetic wisdom" (p. 83) that disclosed the strategies by which the divinely ordained authority of kings could be sustained. Carrying this view even further, Louis Machon (chap. 6) saw Machiavelli as "a misunderstood Christian moralist" (p. 188) whose teachings were perfectly consonant with the Bible, classical political philosophy, and the royal and arcane *kenosis* of sacred kingship. In direct opposition to this perspective, the rebel printer John Wolfe (chap. 3) viewed Machiavelli not as a royalist but as a closet republican whose secret intention was to subvert dictatorial rule by exposing to the people the *arcana tyrannorum*. For both Arnold Clapmar (chap. 4) and Gabriel Naudé (chap. 5), Machiavelli's ideas functioned as a source for, and intellectual bridge between, the *arcana imperii* alluded to by Tacitus and other classical political philosophers and the recently emerged doctrine of reason of state. But whereas Clapmar simply associated Machia-

velli with the dark or immoral side of this tradition, Naudé regarded him as a more complex thinker, wrestling with the mysterious process by which evil was transmuted into good in political life.

Unfortunately, Donaldson ends his book abruptly without trying, in a formal conclusion, to summarize his findings or their importance for understanding Renaissance political discourse. But the core factor limiting the utility of this work is that it explores an implausible view of Machiavelli's thought developed by secondary thinkers who, in most cases, did not, as Donaldson himself indicates, systematically or carefully read *The Prince* and *Discourses*.

While conceding that "efforts to read Machiavelli as the reviver or reviser of an ancient mystery of statecraft now seem strained," Donaldson nevertheless argues that "the facts concerning publication of his major works, as well as their subject matter, offered a foothold for such a reading" (p. ix). But this tenuous foothold, if it actually exists at all, quickly crumbles under the weight of any serious scrutiny of Machiavelli's principal texts. In the introduction to the first book of the *Discourses*, Machiavelli expresses his intention "to open a new route, which has not yet been followed by anyone." And in the dedication of *The Prince*, he declares that he wants to convey "knowledge of the deeds of great men . . . acquired through a long experience of modern events and a constant study of the past." Looking at Machiavelli's explicit teachings, it is hard to understand how they were ever construed as revealing an ancient tradition of secret political instruction when they were obviously distilled to a substantial degree from the deeds of an incredibly varied lot of Renaissance political actors—not only "sacred" and kingly figures like Charles of France and Pope Julius but also freebooters and ruffians like Oliverotto da Fermo and Cesare Borgia.

Machiavelli and Mystery of State reflects a deep and wide knowledge of the primary sources of Renaissance political literature and discourse. But it is a work for a specialized audience of political theorists who are particularly concerned with either the six figures on whom Donaldson focuses or early interpretations of Machiavelli's thought.

JOHN LANGTON

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Yankee Red: Nonorthodox Marxism in Liberal America. By Robert A. Gorman. Westport, CT: Praeger Publishers, 1989. 208p. \$39.95.

Written in engaging prose, *Yankee Red* is a useful addition to the list of books on U.S. radicalism. Not content to rehash the question of why there is no socialism in the United States or to lament the alleged disappearance of radical intellectuals, Gorman analyzes the history of recurring efforts "to reformulate Marxian theory and practice to accommodate America's idiosyncratic individualism" (p. 14). Focusing on leftist activists and theorists who have diverged from, or directly challenged, orthodox Marxism and briefly assessing their contributions to social thought and social change, Gorman has found that U.S. neo-Marxism has generally been rich in practice but poor in theory (compared to the European variety).

Specifically, Gorman asserts that "nonorthodox Marxism in America has crystallized around four important components of the liberal worldview: the hegemony of conscious subjects; the state as impartial arbiter of diverse interests and politics as an apt means of transforming society; science as systematic empirical inquiry; and the pluralist belief that policy formulation is characterized by conflict between multiple, competing social interests" (p. 14). The book thus examines the diffuse character of U.S. neo-Marxist thought through its approach to each of four themes: radical subjectivity, proletarian politics, empirical science, and progressive pluralism. In each instance, Gorman first sketches the familiar (if not stereotypical) positions of orthodox Marxism, then provides a thumbnail history and theoretical analysis of key figures in the development of nonorthodox Marxism. For example, the chapter on subjectivity covers a range of movements and thinkers from the Wobblies to the New Left, from Eugene Debs to Sidney Hook. Other chapters similarly devote attention to such thinkers as Morris Hillquit and Michael Harrington, Alvin Gouldner and Erik Olin Wright, Adam Przeworski and Theda Skocpol, W. E. B. Dubois and Manning Marable, Shulamith Firestone and socialist feminists, and Reinhold Niebuhr and liberation theologians.

Gorman's basic argument is the sound (but often overlooked) one that Marxist theorizing, to bring about socialism successfully, must

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adapt itself to the major features of U.S. political culture: "a rock-bottom sense of individualism, a faith in political activism, a commitment to empirical science, and a tolerance of deep, powerful noneconomic associations [i.e., race, sex, and religion]" (p. 170). These are indeed the very aspects of our political culture to which nonorthodox Marxists and other radicals must appeal and that account for the theoretical vicissitudes characteristic of U.S. radicalism.

As a sort of hitchhiker's guide to U.S. neo-Marxism, Gorman's book is an indispensable survey of radical thought and a helpful map for further research. Its analytical framework—focusing on the broad political themes that have been, and must be, incorporated into Marxism for radicals to be successful—should help the book find an audience among both undergraduate and graduate students in courses on U.S. political thought. Specialists, though their thinking will be stimulated by Gorman's analysis, will ultimately be left unsatisfied by the pace and brevity of the treatments of distinct movements and thinkers.

Although the individual descriptions and analyses are fair and accurate, the brief portraits and commentaries Gorman provides will no doubt seem simplistic and highly stylized to many specialists. For example, the complexities in the thought of the New Left or Michael Harrington, let alone the vagaries of contemporary Marxism, deserve a more extensive treatment (e.g., three pages for the New Left, seven for Harrington) than the grand sweep of this book is able to provide. The book also contains a few minor errors that detract from its otherwise excellent ability to engage the reader: *flourishing* occurs in place of *flourishing* a number of times, and there are such misidentifications as labeling Wittgenstein's work as the *Logical Investigations* and the IWW as the "International Workers of the World."

Yet for those plagued by the perennial concern of what's left of the Left, as well as for those interested in U.S. political thought, this book is a potent reminder that leftism in general and Marxism in particular never really disappear, they just take on new, ever-more-American forms.

LEONARD WILLIAMS

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The Fatal Conceit: The Errors of Socialism.

By F. A. Hayek. Chicago: University of Chicago Press, 1989. 180p. \$24.95.

The Fatal Conceit is the first volume in a projected 22-volume collected works of the Austrian born economist, political philosopher, and Nobel laureate F. A. Hayek. The series will contain his full-length books, a reorganization of his published essays, previously unpublished English translations of various pieces, and his correspondence with Keynes and Popper. The present volume contains a short but original set of essays prepared over the last 10 years as part of a projected "grand formal debate" with the leading theorists of socialism under the heading "Was Socialism a Mistake?" While the debate never took place, the ammunition Hayek prepared to defend the market order now appears in this volume.

The targets of Hayek's barbs remain the same: naive rationalists, socialists, and social planners. They believe that the market order is chaotic and unjust and wish to replace it with a system of reason and social justice. Readers of Hayek already know of his views on the evolutionary development of spontaneous orders and how the best products of civilization stem from individual actions without those actors knowing how or why their actions contribute to human progress.

But Hayek acknowledges that his defense of the liberal order runs counter to the sacred principles of modern thought: rationalism, empiricism, positivism, and utilitarianism. As he puts it, we have been imbued with a basic set of ideas about what is, and is not, reasonable. It is "unreasonable" to "follow what one cannot justify scientifically or prove observationally," to "follow what one does not understand," to "follow a particular course unless its purpose is fully specified in advance," and to "do anything unless its effects are not fully known in advance but also observable and seen to be beneficial" (pp. 62-63). In dealing with our social order we have been using "false assumptions about the possibility of justification, construction, and demonstration" (p. 71) in restructuring and directing our social arrangements. The result is a conceit concerning the belief that human reason can shape and control the direction of human progress.

In short, these principles (scientism) have been illegitimately used by modern thinkers to

apply the morality of the microcosmos to the extended order of the macrocosmos. In small groups or tribes where all relations are face to face, it is possible to operate collectively for shared goals and concrete ends. But as humankind moves beyond the face-to-face troop and into the extended order with its greater differentiation, individualism, and dispersed knowledge, it is necessary to shift from the morality of the small group (cooperation and altruism) to the universalistic morality of common abstract rules, private property, free trade, and the freedom to pursue individual goals. But the benefits of doing so are not easily understood or appreciated. Hence, we are tempted to operate by designing goals and imposing standards as small groups or families do. However, such goals in the extended order can only be accomplished by the "arbitrary or discriminatory coercion" by those empowered to direct society to those goals. This is the "fatal" part of Hayek's story. As he puts it, "enforced obedience to common concrete ends is tantamount to slavery, obedience to common abstract rules provides scope for the most extraordinary freedom and diversity" (p. 64).

Hyperbole aside, Hayek admits that the advantages of obeying abstract rules and operating in the competitive market order are hard to comprehend, anticipate, or prove scientifically. So how can we demonstrate the liberal order's superiority?

Hayek tries to deal with this issue in chapters 1 and 2, where he speaks of traditions, customs, and morals as evolutionary human products that are neither innate nor the designs of human reason. These practices lie "between instincts and reason" and are maintained and followed because they confer benefits widely to those in the extended order. However, there is great confusion about, and misunderstanding of, the importance of cultural evolution. In the revolt of "instinct and reason" we tend to succumb to the conceit of wishing to design all aspects of social existence. In other words, we become entangled in an intellectual juggling act to clothe the noble savage in the robes of science, hoping to impose the instincts of the small group with the power of comprehensive social engineering. Only a rational reconstruction of these traditions and practices can demonstrate how they have evolved and taken root. When and where practiced, success follows; when not, repres-

sion and poverty remains. The traits of disciplined work, contract, exchange, saving, risk taking, and several property worked because they are best suited to allowing us to prosper and to meet unforeseen events in a complex world.

In the final chapter Hayek makes a rare personal comment on his views concerning religion. As a "professed agnostic" he rejects "every anthropomorphic, personal, or animistic interpretation" of "what others call God" (p. 139). Yet religion has played an important role in helping us maintain beneficial traditions in the extended order. By wrapping traditions and customs in mystical authority, it is possible to preserve and to transmit them when a rational defense of certain practices is unavailable. Historically, support for the family, honesty, and private property have often come from the values contained in religious teachings, making the "blind habits" associated with religion functional in sustaining valuable customs. He fails to note, however, that religion can be, and has often been, an enemy to liberal values.

The Fatal Conceit is a wide-ranging work containing many profound insights about cultural evolution, morals, trade, neo-Malthusianism, and the benefits of the market order. While some will contest many of his broad historical generalizations and will find his views on morality decidedly tilted toward commercial or market values, *The Fatal Conceit* is an important and original work in the classical liberal tradition.

LAWRENCE J. CONNIN

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The Desperate Politics of Postmodernism. By Henry S. Kariel. Amherst: University of Massachusetts Press, 1989. 184p. \$30.00 cloth, \$11.95 paper.

If we have seen the end of what Jean-Francois Lyotard calls the "grand narrative" and the beginning of postmodern history, is there a chance to recover a politics that is open, humane, and functional, given the anti-foundational premises of postmodernism? What kind of politics is possible without natural law, a teleological conception of

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history, and some fixed notion of human nature? The latest work by Henry Kariel, *The Desperate Politics of Postmodernism*, confronts this issue.

In providing numerous examples on behalf of the postmodern perspective in art, literature, and culture, Kariel is engaged in broadening an earlier enterprise begun in *Beyond Liberalism: Where Relations Grow* (1977). In *Beyond Liberalism* Kariel points to the failure of liberal institutions, which isolate individuals and fragment their private and public lives. In place of liberalism Kariel offers an open-ended aestheticism in which artistic performance serves as the model of political action.

In *The Desperate Politics of Postmodernism* Kariel expands this theme in an examination of the crisis of modern culture. He begins with the premise that modern (liberal) political institutions fail to comprehend that political structures are residual components of a larger cultural enterprise. The "political" is simply one more reflection of who we are. In reading "who we are" from "what we have produced" (particularly in the area of art), Kariel seems sympathetic to the claims of the postmodern scholars and artists that the residue of our culture reflects an instrumental nihilism, a technology of control, and a politics of conformity.

The central role of art in Kariel's strategy, however, exposes several problems within the work. Kariel looks to art—particularly postmodern art—and sees the breaking of routines, structures, and boundaries. But Kariel could do more to confront the political question in his methodology. By his own admission postmodern politics operates within the dominant culture of advanced capitalism. If this is a culture that quietly seduces us into accepting our fate (p. 116), can art retain the status that Kariel has established? If Herbert Marcuse, Hal Foster, Walter Benjamin, and others are correct that advanced capitalism absorbs and conditions even art, the role and the status of art need more treatment than Kariel provides.

This problem relates to a more fundamental issue regarding the characterization of the artistic enterprise. As a process, Kariel treats artistic creation as an activity outside of the instrumental routine of the dominant culture. Art breaks the pattern, shifts the ground, and alters the perception of the creator and the consumer. Ultimately, the parameters of what

is real and unreal are changed (pp. 13–14). Kariel draws an implicit parallel between the shifting of reality by the artistic enterprise with the destructuring or deconstructive enterprise conveyed by postmodernism. In a sense, both activities require a certain space, a distance, in order to escape the tyranny of structure.

But has Kariel done justice to the postmoderns, particularly the poststructuralists, in drawing this parallel? The position of the poststructuralist is epistemological. Historical narratives are reduced to fiction because they cannot stand the weight of critical examination. But Kariel's claim for distance seems ontological in character. The creator needs the space in order to engage in the creative enterprise: "The artist allows his belief in reality to dissolve until he can scarcely manage to steady himself" (p. 17). He "escapes" and "surrenders all but consciousness" in order to create his art. But this view of the individual as "subject-creator" is representative of the philosophy of consciousness found in modern romanticism, not postmodern philosophy. Art, as a subjective retreat for the individual, obscures the real crisis conveyed by the postmodern perspective.

The end of structure, the life of uncertainty, the shifting ground of contexts, would certainly lead to a pessimistic appraisal of the human condition. An active pessimism characterizes postmodernism (p. 151). But Kariel is searching for a way out. If a politics emerges that accepts the loss of the grand narrative, more space can be opened up on the political margins. If no one has an exclusive claim to truth, a more tolerant and flexible politics can emerge. If we abandon the search for hypothetical futures, the politics of the present can be open, antidogmatic, and humane, a condition Kariel compares to the "idea speech situation" in Habermas (p. 101).

Kariel finds an analogy to his conception of politics in the notion of "play." Play, as something that is interactive, spontaneous, and probing, is not guided by fixed codes and "rhythms." It is free to experiment, change, and adapt. Play is the antitotalitarian alternative for a modernity that has produced a stagnant repressive bureaucracy, a monolithic productive technocracy, an empirical nihilism, and reactive, directionless, politics. But Kariel is able to draw his optimistic conclusion by not confronting the affinity between instrumental

capitalism and innovation. An open-ended, undogmatic, technocracy is the very notion of "modernity" carried to its logical conclusion. Without considering the relationship among knowledge, narrative, and the existing institutions of power, Kariel is not in a position to do more than indicate that "modernity" is in crisis.

In general, Kariel's latest work needs to be seen as a continuation of his earlier concern for creating an aesthetic politics. But in examining modernity's crisis rather than postmodernism's epistemological challenge, Kariel has missed something. But that is hardly to suggest the work is without merit. Often appearing motionless, the fragmented style reveals a collage of symptoms. In that regard, the book is an interesting and honest perspective on future possibilities.

ANDREW M. KOCH

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Far Left of Center: The American Radical Left Today. By Harvey Klehr. New Brunswick: Transaction Books, 1988. 196p. \$29.95.

What's Left? Radical Politics and the Radical Psyche. By Michael Neumann. Peterborough, Canada: Broadview, 1988. 230p. \$12.95 paper.

Being a radical today is not as easy as it used to be. Until recently, orthodox Marxist-Leninists, preaching the gospel of historical materialism, held a monopoly on the left, funneling prospective radicals directly into the nearest Party outlet. In spite of its successes, orthodoxy proved a mixed blessing, particularly to Western leftists—primarily because it was neither democratic nor individualist enough to attract disillusioned liberal democrats. Renegade radicals soon discovered the roots of a revised, nonmaterialist dialectic. As critical self-consciousness replaced stampeding matter, heretofore-bourgeois theories were coopted by the Left, and radical social change was linked to ideas that were willingly chosen and democratically expressed. History, in short, has turned Marxism into a churning entity comprising orthodox and nonorthodox participants, both nervously cohabiting on socialist turf. Radicals now have a meaningful

choice, which means that they must work harder before committing. Unfortunately, neither Michael Neumann's *What's Left?* nor Harvey Klehr's *Far Left of Center* provides much assistance.

Neumann has written a bitter, retributive polemic that is ostensibly aimed at radicals but actually hits only a lunatic fringe of that heterogeneous movement known as the New Left. Neumann's "radical" is someone more willing to use extreme tactics than nonradicals. By this definition, every fanatical activist qualifies as a radical, regardless of ideology (which, incidentally, Neumann derides as meaningless and superfluous). The intended victims of Neumann's mocking, often-vicious verbal attacks are thus vacuous activists willing to use violence opportunistically. Neumann calls them "rebel" personalities. Although the New Left probably had more than its share of such deformed rebels, in condemning both New Leftism and, more generally, the entire Marxian Left, Neumann is both naive and unfair. "The thought of being a radical," states the ex-New Left author, "makes me sick" (p. 179). Living and working with such people would, I think, make most of us sick. However, reasonable people, Marxist or not, would have bailed out immediately. Neumann apparently didn't and now regrets it. Perhaps he has learned to choose friends more carefully.

I suspect that Neumann is experiencing guilt as much as anger, for he exhibits most of what he views as radicalism's unsavory qualities. He insists, for example, that radicals are moral, intellectual, and cultural snobs. Pertinent evidence is apparently drawn solely from personal acquaintances and experiences in unnamed, atypical locales, where, he outrageously claims, the label *radical* "consistently confers respect on its wearer, even from his enemies" (p. 9). Moreover, he debases middle-class progressives as "morally deficient" (p. 181), rejects political democracy as inefficient (pp. 206–11), and elevates selfishness above altruism and charity: "Organization[s] should make no effort to alleviate the sufferings of others except when . . . [their own] lives . . . will be the better for it" (p. 219). His negative thoughts on theory and morality could easily have originated in a New Left primer. Finally, his belated discovery of truisms first enunciated long ago by nonorthodox U.S. Marxists

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(e.g., "Successful political organizations draw their strength, not from hermetic truths, but from widespread beliefs and desires" [p. 192]) mimicks the tendency among many New Leftists to disregard history. Neumann concludes cryptically by advocating a new kind of radicalism that is nonideological, nonthematic, selfish, elitist, and, wherever possible, non-radical.

Harvey Klehr's *Far Left of Center*, on the other hand, is a scholarly, clearly written description of the ideology, structure, and leadership of the major extant Marxist-Leninist parties in the United States. It gathers factual information from a variety of traditional and nontraditional sources into one convenient, encyclopedic volume, which scholars can quickly and easily utilize. This is not the kind of book that one devours from cover to cover. Consequently, most readers will probably not fully appreciate Klehr's high level of scholarly competence. Nor will they experience the disquieting cumulative effects of the author's questionable presuppositions.

Klehr feels that Marxism-Leninism presents a real threat to democratic institutions, "one that is overlooked or minimized to the peril of the security and best interests of the American public" (p. xii). He devotes two-thirds of his book to describing the Communist Party of the United States of America as well as Trotskyist, Maoist, and miscellaneous Communist parties, in the hope that this kind of knowledge, broadly disseminated, will forewarn and protect the public. Klehr also asserts that the orthodox Left in the United States is anti-Semitic (the book was funded and copyrighted by the Anti-Defamation League of B'nai B'rith) and that the United States' "broader, noncommunist radical left" (p. 131), by cooperating with communists in opposing specific issues and policies of the U.S. government, provides "a moral seal of approval" (p. 131) for communism and hence is equally culpable.

Despite his best and most energetic efforts, Klehr presents not one shred of verifiable evidence that any United States-based leftist party is anti-Semitic. What he amply documents is the orthodox Left's opposition to Israeli policies (and in surprisingly few cases to the existence of the Israeli state itself) and its support for a Palestinian homeland. Bigotry, of course, shouldn't be tolerated. But in a pluralist democracy people will often disagree

on significant policy issues. Klehr advertises the advantages of liberal democracy but won't buy its disagreeable consequences. Opponents of Israeli and U.S. policies are not necessarily anti-Semites.

Even more troubling is Klehr's indictment of noncommunist organizations like Jesse Jackson's Rainbow Coalition, Clergy and Laity Concerned, Committee in Solidarity with the People of El Salvador, and the Institute for Policy Studies. Their occasional, temporary, and shifting alliances with lawful communist front organizations on legitimate moral and political issues does not transform them into communists or anti-Semites. Even Klehr admits this (e.g., pp. 131-132, 144, 152, 175). Coalition politics has always flourished in the liberal United States. Why, then, does Klehr consider these organizations "enemies" of democracy? And why does he continually list names of noncommunists—including several current Democratic politicians—who have lectured, contributed articles to, or indirectly cooperated with, one or another leftist organization? If U.S. freedom is threatened by those who peacefully oppose certain U.S. and Israeli policies or who question the utility of capitalism, perhaps our real enemies don't live on the left.

ROBERT A. GORMAN

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Toward a Feminist Theory of the State. By Catherine A. MacKinnon. Cambridge: Harvard University Press, 1989. 320p. \$25.00.

MacKinnon's book grapples with the meaning of politics and how we think about what constitutes the political in terms of sexuality itself. The first section of the book, "Feminism and Marxism," presents published materials from the mid-1970s, which she says may seem "groping and comparatively primitive" (p. x). Anyone who has not already read these articles (which are slightly revised) will find them theoretically significant and important contributions. For those familiar with the debates between Marxism and feminism this will be a review of familiar territory. Much has changed

over the past decade in this debate due to developments within feminism itself. An accounting of these developments would begin to uncover some interesting transformations that have located many socialist feminists of this earlier period outside the debate today.

The second section of the book discusses the method and epistemological foundations of feminism and the politics of sexuality. MacKinnon, like many radical feminists, argues that the feminist method and politics of "consciousness raising" uncovers women's situation "as it is lived through," as "lived knowing." Through this process women come to know politics differently—in a different way—because the process of knowing uncovers its own politics. Coming to know the "personal as the political" reinvents the way we think of politics itself because "the personal is epistemologically the political, and its epistemology is its politics" (p. 120).

In the consciousness-raising process sex and gender become interchangeable for MacKinnon, but they do not remain equivalents. Instead, sexuality becomes the linchpin of gender inequality (p. 113). MacKinnon's feminism identifies sexuality as the primary social sphere of male power through rape, incest, battery, sexual harassment, prostitution, pornography, and abortion.

In the third section of the book MacKinnon constructs a vision of the state that theorizes sexuality at its base: state power emerges as male power: "The state is male in the feminist sense: the law sees and treats women the way men see and treat women" (p. 162). And the "male perspective is systemic and hegemonic" (p. 114). She asks what state power is for women, how women encounter the power of the state, and how law legitimates male power. She also begins to uncover how the state embodies and serves male interests. This final section of the book points in important directions as it demands a recognition that male privilege is systemic and is organized through the construction of the state.

The discussion put forth about feminist method and politics is crucial for any contemporary understanding of politics today. MacKinnon convincingly argues that gender is a system that divides power and constructs politics. But her analysis of male power and the state appears overly determined and homogeneous. The feminist notion of the "per-

sonal as the political" and the idea that there is a "politics to sex and gender" is the unique and valuable contribution of radical feminism to feminism (and to political theory).

But since the development of this notion in the 1970s, there have been crucially important feminist critiques (through the 1980s) of the limitations of this notion. The critiques have uncovered problems with equating (rather than relating) personal and political life and with treating sex and gender interchangeably. Although the realms are completely implicated with each other, they are not simply interchangeable realms. We further elucidate politics when we can explain how the two are similar *and* how they differ.

Although I would agree with MacKinnon that male power is systemic, I do not think it is internally homogeneous and undifferentiated, as she seems to. The state institutionalizes male privilege. But patriarchy as a system does not merely embody male privilege in terms of sexuality, as male bodies. Gender privilege has a complicated set of structures and institutions that cannot be reduced to maleness *per se* or sexuality.

The politics of the state expresses different factions with both similar *and* differing interpretations of male privilege. Clearly, this is evident in the abortion debate today with the varying reactions within state legislatures like Florida, Georgia, and Pennsylvania or in the presidential veto of the House and Senate proposal on broadening Medicaid funding.

MacKinnon does not direct her inquiry here because she rejects the theoretical premise that the state is simultaneously patriarchal and structured by liberal discourse and that these realms remain distinct. She equates liberal feminism with liberalism applied to women and socialist feminism with traditional Marxism. She equates the state with male power; hence, she posits the centrality and homogeneity of sexuality. In this scenario, the politics of abortion *only* reflects male power.

In the end, she argues that radical feminism (which she terms "unmodified" feminism) is feminism. But the richness of feminism today is that it has emerged out of a *transformed* Marxism, a *revised* radical feminism, and a liberal feminism that has uncovered its own limitations via the critiques of women of color, radical feminism, and so on. The source of feminism's radicalism today is that it has been

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(radically) modified through a dialogue between feminisms. The critiques of feminism by feminisms have refined our understanding of the *relationship* between the personal and the political. They cannot be equated or collapsed as one.

MacKinnon knows better than most that there are differences between the personal and the political. But her presentation of feminist epistemology and her theory of the state do not give us any room to construct what they are.

ZILLAH EISENSTEIN

Ithaca College

The Libertarian Idea. By Jan Narveson. Philadelphia: Temple University Press, 1988. 367p. \$34.95.

Freedom and Authority. By William McKercher. New York: Black Rose, 1989. 300p. \$36.95 cloth, \$16.95 paper.

Narveson's book is an attempt to rescue libertarianism from atomism and to place it more firmly in contractarian moral theory. After outlining the theoretical foundations of libertarianism, the author defends its coherence against the attacks of selected critics. In brief, Narveson's intention is to place libertarianism on a more solid footing, namely, on Hobbes's contractarianism with the assistance of David Gauthier by way of a running assault on Nozick. The author, who styles himself a "professional philosopher" writing for "professional philosophers" and students of philosophy, divides his book into three parts, the third of which—on the application of libertarian theory to concrete political and social issues—may interest political scientists who wish to understand the source of an increasingly influential pop philosophy. Narveson claims to write "in the sober . . . prose of philosophical analysis." In fact, the book is enthusiastic, at times chatty, and occasionally spiced with autobiographical asides.

It is, above all, an emotional (not to say arrogant) book. The emotion that dominates the whole is impatience. Narveson has little or no patience with his great predecessors, such as Hobbes and Locke, or his contemporaries, such as Nozick. He promises at the outset that the "reader will not find here a systematic delv-

ing into the works of the many libertarian writers" (such as we find in McKercher's book). Indeed, the reader finds no systematic delving into any writers. Narveson's impatience leads him to give a "more or less" accurate account of the great philosophers such as Aquinas, Locke, Hobbes and even Kant. His account of these philosophers is sometimes astonishingly inadequate. For example, he blames both Aquinas and Locke for "their lack of articulation of the concept of natural law." So much for Aquinas' treatment of natural law in the *Summa theologiae* (secs. 90-104) and Locke's *Essays on the Law of Nature* and discussion in the *Second Treatise of Civil Government*. It is very clear that Narveson travels light; he is not weighed down with the baggage of philosophical authority, at least none farther back in time than David Gauthier.

Serious students of political philosophy will not find this book useful except as an illustration of the rootless malaise into which contemporary philosophy has wandered of late.

McKercher's book, by contrast, is rooted in the writings of a whole array of nineteenth-century writers such as William Morris, W. Donisthorpe, Edward Carpenter, J. M. Davidson and G. A. Aldred (who, we learn from the 22-page bibliography, wrote 27 books on or around the subject of libertarianism). In a very real sense this book tells us much more than we could possibly want to know about the nineteenth-century roots of the British Socialist-Libertarian-Communist movement.

From McKercher's book we learn that libertarians make an important distinction between "command authority" and "belief authority" and that libertarians are against the former and for the latter. That is to say, they are against the form of authority "which they believe to be responsible for the 'artificial' system of economic tyranny which deprives the majority of the populace of a more complete form of freedom." On the other hand, "their theories rest upon a highly complex system of belief-authority."

When he comes to treat the content of the freedom championed by libertarians, we are left with very little of substance. We learn only that libertarians wish to be "free to do those things which they are at liberty to do." This clearly has a tinge of circular reasoning. In the final analysis, freedom for libertarians is a

fence around a vacant lot. If set against the great political philosophers of the classical tradition (the author does not venture to make such a comparison) libertarians pale into insignificance. There is no concept of virtue, intellectual, moral, or political. Despite his efforts to the contrary, libertarianism lapses into nihilism. There are no standards by nature—the libertarian man makes his own standards. The author tells us that “the libertarians viewed authority, law, and the state as restrictions generating an immoral state of society which needed to be totally changed.” We are not told what a moral state would look like. But perhaps this is more the fault of libertarianism than of McKercher.

It should be clear that these two books do not engage in a dialogue. Narveson uses a vague account of libertarianism as a vehicle for his personal thoughts on a range of contemporary issues without any effort to locate those thoughts in the libertarian literature. McKercher, on the other hand, all the while steeped in nineteenth-century British literature (especially the writings of William Morris), shows no concern for pressing contemporary issues.

FREDERICK VAUGHAN

University of Guelph

Socrates and the Political Community. By Mary P. Nichols. Albany: State University of New York Press, 1987. 237p. \$49.50 cloth, \$16.95 paper.

The crucial decisions of statesmen who attempt to enable a citizenry to partake in the blessings of “the good life”—whether by fostering virtue or merely by providing the conditions of security—necessarily involve the consideration of particulars. Careful attention must be paid to the special characteristics of a particular geopolitical situation, particular men and women, a particular culture and political experience, a particular climate, and so on. At the same time, however, the goal at which these decisions aim has a universal scope. One who understands the ultimate end pursued in politics to be justice, liberty, equality, brotherhood, eternal salvation, personal aggrandizement, or anything else believes, in principle, that this end is best for everyone.

The very definitions of these ends will tend, accordingly, to discount the differentiation and particularity of the human situation. The luster of these universal or cosmopolitan goals would seem to depreciate the unique importance of the particular person and his station in family and national life. As in the case of the proverbial philanthropist who loves “humanity” but detests his neighbors, particularity is homogenized to wrench the universal into existence. It is the important contribution of Mary Nichols’s *Socrates and the Political Community* to connect this problem of the homogenization of persons (which is nothing other than a pathological deformation of *phronesis* or practical wisdom) with the mathematical orientation of political education in Plato’s *Republic*. Nichols’s sensitive and intelligent interpretations of Aristophanes’ *Clouds* and book 2 of Aristotle’s *Politics* respectively introduce and complete her discussion of this problem. Nichols maintains (1) that the controversy between Aristophanes and Plato centers on the question of whether Socratic philosophy tends of its own nature toward this mathematical homogenization and (2) that it is only in Aristotle’s account that one finds the consummate statement of the problem of the vulnerability of political judgment to mathematics’ facile competence at revealing “universals.”

For the sake of the general reader it should be pointed out that the title of Aristophanes’ *Clouds* refers to certain vaporous, cosmopolitan deities, which Socratic inquiry (according to Aristophanes) elevates above the personal, civic gods of Athens. This alleged theological innovation in fact had a part in the indictment under which the historical Socrates was eventually found guilty and executed. In the play, the poet sees to it that events unleashed by the Socratic teaching punish Socrates’ disregard for the precious and particularistic foundations of human happiness. The sapience of Nichols’s reading of Aristophanes is evident in her rejection of the customary invocation of Socrates’ so-called “second voyage” as a means of exempting the Platonic Socrates from Aristophanes’ criticism. At one point in his life Socrates renounced the study of natural philosophy in favor of the examination of speeches, the latter being the “Socratic method” (with which we are familiar thanks largely to the dialogues of Plato). He describes

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this resolution, in an autobiographical digression in the *Phaedo*, as his "second voyage." By spurning the wide and easy way of reconciliation and presenting an accurate and unadulterated Aristophanes, Nichols pioneers an explanation of Aristophanes that furnishes an important insight not only into the dubious escapades of the "pre-Socratic" Socrates but also into the serious problems raised by the inquiries of "our hero," the Platonic Socrates. Implicitly, then, Plato's *Republic* is not to be viewed merely as a response to the charges of Aristophanes, Socrates' "chief accuser." In a sense, the *Republic* is best understood as itself a party to the indictment.

Nichols's interpretation of the *Republic* is innovative and incisive. Everyone knows that Socrates was tried and found guilty of corrupting the city's youth. But Nichols contends that a careful consideration of the dialogue indicates that it is the philosopher (read the philosopher king of the city-in-speech) who is corrupted by spirited youth. High-spirited politics, owing to its preoccupation with the perfect realization of justice, mutilates philosophy. The deformed philosopher apes the activist's contempt for diversity and heterogeneity as obstructions to his project of bringing heavenly justice down to earth. As a result, the "sights" this philosopher contemplates are empty abstractions devoid of reality and beneficence. Hence, Nichols starkly contrasts Socratic political philosophy (what Socrates himself practices in the dialogues) with the politicized and desiccated philosophy connected with the nurture, education, and government of the "philosopher king." In detailing this contrast, Nichols attacks the mathematical curriculum Socrates institutes as the cornerstone of philosophical education in the city-in-speech. She interprets mathematics as the consummate symbol of the empty, homogeneous abstractions with which deformed or "spirited" philosophy concerns itself. She then contends, quite elegantly, that Plato answers Aristophanes' public-spirited critique of philosophy by showing how tendencies within the political spirit itself are to blame for the corruption that philosophy seems to introduce into the political community. As the author's vivid elaboration of this thesis demonstrates, genuine insight into universal truth is hardly abstract. Nevertheless, in the end it seems she would join Aris-

tophanes in the indictment of Socrates. Nichols confesses that even the uncorrupted Socratic philosopher can do no more than leave men in a state of *aporia* (doubt). Perhaps revealing the influence of her own spiritedness, she then concludes that "even this philosophy is too abstract. If it leaves men in a state of *aporia*, it leaves them 'in the clouds'" (p. 149). In accordance with this assessment of *aporia*, Plato's account of the relationship between philosophy and politics is found inadequate.

To supply this inadequacy, Nichols turns finally to Aristotle. Book 2 of the *Politics* presents, above all, Aristotle's criticism of the political constitution outlined in the *Republic*. Rather than treating this criticism in isolation, however, Nichols skillfully relates it to Aristotle's criticisms of Hippodamus' excessively geometrical polity, Phaleas' limited vision of man as *homo economicus*, and the Spartans' one-sided devotion to a spirited manliness as the sum of all virtue. The result is a compellingly synoptic view of Aristotelian principles of constitutional criticism. The author maintains that these principles provide a basis for the successful reconciliation of politics and philosophy and thus of human particularity and the universality of virtue and happiness. Aristotle's unique understanding of the necessity of a certain diversity both in the order of being and in the order of politics properly orchestrates the partial truths noted by Aristophanes and Plato. Moreover, as the author points out, Aristotle's political philosophy provides for the healthy integration of the schizoid tendencies of modernity, which vibrates between the reification of the opposite abstractions of individualism and communism. Both considerations testify to the soundness of Aristotle's political philosophy.

Of course it is characteristic of so thoughtful, original, and challenging a work as this that it should provoke some critical questions. One wonders, for example, if it is Aristophanes' view that the search for first causes necessarily ends in the discovery of "whirl"; that is, does Aristophanes agree with his Socrates as to this "truth," only to disagree concerning its publication? Or is it Socrates' willful substitution of vacuous abstractions for personal gods that is the precise target of Aristophanes' ridicule? Supposing the latter, one may inquire how Aristotle—whose own conception of god is hardly more personal than

Socrates'—could provide an answer to Aristophanes' critique of Socrates that Aristophanes would find satisfactory. Additionally, one might wonder just how far *ta mathemata* may be taken as symbolic of a vacuous homogeneity in the *Republic*. Does Plato's Socrates regard the *eidos* (form) as an abstraction or as some thing, not merely a name? Further, if what is most remarkable in the "universal" square and circle is their incommensurability—something absolutely inaccessible in particular squares and circles—then "pure" mathematics necessarily draws attention to the most fundamental heterogeneity. Given the respectable ontological status Socrates accords to mathematics and its striking capacity for the symbolization of heterogeneity—to say nothing of its capacity to foster psychic *periagoge* (turning round)—might it not belong to a curriculum truly appropriate to philosophy? And if it is the case that the study of mathematics, thus understood, is emblematic of the "philosopher king," on what basis might one maintain the latter's opposition to the philosopher Socrates?

DARRELL DOBBS

Marquette University

Recovering the Social Contract. By Ron Replogle. Totawa, NJ: Rowman & Littlefield, 1989. 253p. \$34.95.

There is a growing sense that the theoretical enterprise of deriving political principles from a social contract understanding of politics is philosophically flawed and politically biased. In particular, contemporary individualist approaches to contract theory are thought to build from an incomplete conception of human agency (an impoverished, ahistorical, unencumbered self) and to smuggle the dominant ideals and practices of Western, liberal societies into their presumably general vision of political life. Ron Replogle accepts that these and related difficulties represent a deep and serious challenge for social contract theory. In the face of such problems and without avoiding the most serious and fundamental issues, he argues that a (reconstructed) rights-based, social contract theory offers the

proper theoretical grounding for our commitment to equal liberty for all.

His attempt to recover the social contract is philosophically informed and theoretically imaginative. He argues that conceptions of the social contract offered by contemporary theorists (Rawls, Nozick, Gauthier, Harsanyi) are, indeed, fundamentally flawed. But their failure to present a viable contract theory results from their inadequate conception of agency—of agents as utility maximizers. The thinness of this theory of agency has led to the undoing of contract theory. By contrast, he believes that classical contract theorists (Hobbes, Locke, Rousseau, Kant) employed a more adequate view of human agency, a view grounded in liberty as independence.

The importance of Replogle's project becomes apparent when we seriously confront the deep tension that is built into liberal, individualist conceptions of social and political life. Individuals are assumed to have a basic right to choose and to pursue their own purposes. Yet they are obliged to respect certain principles of political order. In particular, the idea of negative liberty (the absence of restraints) treats virtually all expressions of individual interest as legitimate and equivalent, with the result that any limits to the pursuit of individually defined goals are exceedingly difficult to justify. This difficulty comes to the forefront with our heightened realization that with respect to the problem of political obligation, the optimal choice for individuals is to be free riders, steadfastly pursuing their own interests while encouraging others to be law-abiding citizens.

Different conceptions of the social contract represent the primary theoretical strategy devised by modern theorists of liberty in addressing these problems. From Locke to Nozick and from Rousseau to Rawls, principles of a properly constructed social contract have been thought, theoretically and practically, to reconcile individual freedom with political obligation. Replogle alleges that the thin theory of agency adopted by contemporary contract theorists prevents them from achieving this critically important reconciliation. Because they fail to distinguish among constitutive (universally fundamental), differential (fundamental to particular persons), and elective (desired but optional) goods, Replogle believes that Rawls and other con-

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temporary contract theorists are unable to provide sufficient normative force for their principles. These principles and the ideals they represent need to be understood in their relation to the concept of a person. The fundamental values proposed by contemporary theorists as the basis of the contract (Rawls's primary goods) are not warranted by their view of human agents as utility maximizers. The resulting principles, disassociated from a strong conception of what constitutes persons, are an insufficient basis for contractual agreement and offer scant hope of receiving general support. It is little wonder, then, that to Replogle the dispute between Nozick's libertarian and Rawls's welfare liberalism simply mirrors the partisan disagreements that prevail in everyday politics. With liberty-as-independence understood as the constitutive value of human agency, he believes a social contract can be constructed that yields a neutral and compelling set of political principles.

For these reasons, Replogle's analysis should be closely studied by those seeking to defend and extend the philosophical project of liberal individualism. He offers powerful reasons for viewing social contract theory as the grounding of rights-based equal liberty. Yet his argument will be less compelling to those who question liberal interpretations of individualism. He is forced to rely on essentially *a priori* claims about the constitutive character of persons in order to overcome the thin theory of agency (finding support for this approach especially in Hobbes); and his independent agents remain too far removed from their history and culture to speak adequately to the contextual basis of agency offered by Alasdair MacIntyre, Charles Taylor, and others. If we admire his ambition to overcome the relativism that seems to have settled in on normative theory, his success in doing so is far from clear. Still, Replogle makes an important contribution to the theory of individual liberty by recovering earlier ideas to build a stronger and revitalized social contract. And he does so while fully recognizing, with Rousseau and other classical contractarians, that after women and men have left the state of nature and entered society, freedom must be identified with a sense of moral agency that is found *within* the principles and arrangements of political order. It is a theoretical message that

both contemporary contractarians and citizens need to hear.

LARRY M. PRESTON

Northern Arizona University

The Ancients and the Moderns: Rethinking Modernity. By Stanley Rosen. New Haven: Yale University Press, 1989. 236p. \$22.50.

Stanley Rosen's work charts a complex and provocative path within the contemporary debate on modernity. His proposal for a "modest defense of Enlightenment" is pitched between and against two anti-Enlightenment constituencies: conservative defenders and advocates of antiquity, and postmodern critics. According to Rosen, each group is reacting to an extreme version of the modern paradigm and consequently each also imbibes a self-refuting extremism of its own. Conservative advocates of the ancients invoke notions of virtue in ways no self-respecting ancient would countenance, while "postmodernism is the Enlightenment gone mad" (p. 20). In other words, each anti-Enlightenment position is fundamentally self-contradictory and therefore dishonest. In revealing the base dishonesty within each position, Rosen seeks to save the ancients from modern conservatives and to dedifferentiate postmodernism by making it an instance of Enlightenment "extremism."

The paradigm of Enlightenment that Rosen extricates, salvages, and defends from its extreme version is articulated as follows: "It is always better to know than not to know, in spite of the fact that knowledge may be put to evil use. . . . The more knowledge we possess, the more powerful we are" (p. 20)—"knowledge" here includes moral and aesthetic sensibility. Furthermore, "It is noble to strive for the increase of human power, not in the vain desire to become gods but in the reasonable desire not to be slaves" (p. 20). His rhetorical strategy here is to invoke ancient concerns and conceptual-ethical distinctions in the service of Enlightenment. Like Kant and Foucault, Rosen defines Enlightenment as an attitude rather than as an epoch. Hence, the impulse to Enlightenment, defined as "wonder and the desire to know" (p. 14), may be found in the

ancients; and modernity is judged to be superior to antiquity because of its "greater nobility": "Modernity is not grounded in a rejection of nobility; it is grounded in the charge that the ancients lacked nobility" (p. 14). According to Rosen, "the truly striking distinction to be drawn is not between the ancients and the moderns, but between the philosophers and the nonphilosophers, or at least between those philosophers who attempt a rigorous defense of political life against decadence, and all those, philosophers and non-philosophers alike, who sing to a more relaxed Muse" (p. 11).

The essays in this collection, some of which have been previously published, are rigorous and spirited performative instances of just such singing. In the service of his relaxed Muse, Rosen covers important philosophical territory. His essays critically dissect thinkers such as Descartes, Fichte, Kant, Hegel, and Nietzsche, uncovering (and perhaps also constructing) instances of the extremism of Enlightenment that provide fodder for anti-Enlightenment critics. Enlightenment thinking that overpronounces its difference from, and victory over, the ancients is shown to be hostage to creeping Platonism, the very stuff of its ostensible disavowal; while Plato himself is rescued from the anti-Platonic Derridean rhetoric of presence-as-domination. According to Rosen, the Platonic philosopher is present only as an absence, for he cannot live within the city as a philosopher.

Partisans of contemporary postmodern theory will take issue with Rosen's reading of Nietzsche, to whom two essays are devoted. We can best make sense of Nietzsche, argues Rosen, "as a product of the very Enlightenment he purports (for the most part) to castigate" (p. 207). Fans of Richard Rorty will be surprised to learn that "Rorty, although he does not know it, is a Platonist" (p. 181) due to his "antitranscendental transcendentalism" (p. 180). These essays on postmodern anti-Enlightenment are sure to provoke. Rosen's immodest defense of a "modest" Enlightenment is pursued with unrelenting vigor against his "extremist" opponents.

In rehabilitating and advocating a notion of *moderation* that is no hand-me-down from antiquity, Rosen invoked this virtue within the frame of modern, humanist territory, that is, with reference to a social and political universe

in which humanity faces its own self-made present courageously. Modesty and prudence—also derived from an ancient lexicon within which it would be folly to take our bearings today—are also rehabilitated by Rosen in the service of Enlightenment, for they "take their natures from the paradigms they serve or the goals to which they are directed" (p. 21). Our paradigms and goals simply are not those of the ancients; nor should they be. What Rosen is willing to import from the ancient to the modern context is an appreciation for "the intimate connection between reason and the good" (p. 20).

Some will take issue with Rosen's nearly exclusively philosophical rendition of "the crisis of modernity"; others will find "prudent enlightenment" insufficiently attuned to the political disputes, struggles, and issues of the age; still others will find helpful grist for the discursive mill of contemporary theoretical efforts to diagnose what ails us.

CHRISTINE DI STEFANO

University of Washington, Seattle

Maternal Thinking: Toward a Politics of Peace. By Sara Ruddick. Boston: Beacon, 1989. 291p. \$24.95.

Maternal Thinking develops a philosophical understanding of society through a careful exploration of the activity of mothering. Sara Ruddick's argument opens with a focus on the concrete activity of mothering, moves forward by developing a model of maternal thinking, and then takes a maternal standpoint to "criticize the destructiveness of war" and to begin "developing peace politics" (p. 12). Her social analysis relies on a praxis epistemology that connects reason, emotion, and action. Working primarily within the tradition of philosophy and feminist theory, she develops a maternal standpoint built on Nancy Hartsock's feminist standpoint. Ruddick's focus on maternal activity enables her to open up new ways of thinking about metaphysics, cognition, and virtue in which concern for protection, nurturance, and training receive primary attention. She describes her purpose: "Throughout, my aim is to articulate distinct ways of thinking about the world—for example, about con-

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trol, vulnerability, 'nature,' storytelling and attentive love" (p. 12).

This book brings together three important contemporary political issues. First, it develops an epistemological strategy consistent with contextual and antifoundationalist truth models. Ruddick's approach, however, includes women, which makes an important contribution to this conversation. Second, she takes seriously the model of care developed by Carol Gilligan and others, while thoroughly desentimentalizing "motherhood," explaining how "maternal peacefulness is a myth" (p. 217). Ruddick moves the care model toward the center of political discourse by showing how it can be profitably used in articulating a nonviolent peace tradition. Her understanding of feminist politics is that it is not about solidarity in oppression but about solidarity with "women in particular situations of struggle" (p. 240) to name abuse and violence and eliminate them. Third, she develops a political ethics that uses maternal thinking to link feminist politics with the nonviolent peace tradition. She makes use of Gandhi's model of peace, among others, to work out an understanding of political action that takes account of the body, sexuality, violence versus nonviolence, and mortality. Those interested in contextual models of truth, in feminist theory (especially within feminist moral discourse), or peace politics will find this book provocative and informative. Readers who are inclined to discover politics only in heroic tales of war or in presidential action may find the examples trivial. Ruddick has chosen to speak about philosophy in familiar terms, telling "maternal tales" to show how they can reshape political thought.

Ruddick fails, however, to provide sufficient examples of policies and practices to enable her readers to put into practice the full implications of maternal thinking. While her philosophical vision necessarily avoids a programmatic approach to politics, without some concrete practices, it may be too easy for readers to enjoy the book while failing to respond to the demands it makes on their lives. She simply says too little about what actions her work empowers citizens to take and gives too little guidance as to how maternal thinking can reshape institutions, practices, and public policies.

Readers should pay close attention to Ruddick's desentimentalizing of "motherhood"

which puts it in the realm of practical realization and should forestall any criticism of it as impossibly ideal. They should also note that Ruddick takes pains to show how her concept of mothering includes men. Thoughtful readers will see the political significance of this issue to all citizens.

Ruddick's resources include figures from philosophy, novelists, personal accounts of war experiences and mothering, and her own experiences of being a mother. She uses poets and novelists to heighten her readers' emotional understanding of complex philosophical issues and straightforwardly draws from black feminist analyses and artists. Through personal accounts, she shows readers the poignant stories of war and the pain in child-parent relationships. In blending these diverse sources, she does not simply tack emotions onto political philosophy but instead brings the reader into her own integrated world. The book is easy to read but asks for hard thinking. Its well-made prose and rich examples will make it quite effective in the undergraduate classroom and in the hands of political activists as long as readers understand that this is not a book for mothers only. It is a book designed to help citizens rethink political perspectives and personal relationships. Ruddick herself sees this work as a beginning point for developing a "feminist maternal politics of peace" (p. 244). The important political issues raised by the text can vitalize conversations among political theorists, especially those interested in feminism, peace politics, and contextual models of truth.

ELOISE A. BUKER

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Hegel's Critique of Liberalism: Rights in Context. By Steven B. Smith. Chicago: University of Chicago Press, 1989. 246p. \$29.95.

Steven Smith has made a lucid and learned contribution to the interpretation of Hegel and to the ongoing effort to introduce Hegel's insights into the current revival of political thought. His book is broader than its title indicates: in addition to Hegel's critique of liberal thought and putting "rights in context," Smith treats Hegel's response to the dilemmas of

post-Cartesian modernity, delves into Hegel's logic, and discusses his philosophy of history, including the quasi-Kojévian end-of-history thesis of recent popular flurry. To approach the book's contents from another perspective, Smith gives us interpretations of Hegel's early writings (chap. 2), the essay *Natural Law* (chap. 3), the *Philosophy of Right* (chaps. 4, 5), the *Phenomenology's* dialectic (chap. 6), and the *Philosophy of History* (chap. 7), surrounding them with an introduction and a conclusion that connect them to the troubles of liberalism and modernity. He presents, in other words, an extensive interpretation of chief elements in Hegel's political thought, linking Hegel's politics both to his intellectual development and to his dialectic and his view of reason in history. Throughout, the tone is moderate, opposing interpretations stated, and arguments pro and con adduced; and a reasonable, centrist Hegel emerges as a "middle ground" between liberalism and communitarianism.

For the student of Hegel, the book has many values. Smith writes with a pellucid style. He interprets Hegel in a sound, coherent, and civil fashion. He moves easily to intelligent characterizations unburdened by Hegelian language, as when he suggests that the aim of the dialectic "is to reconcile, to mend, and to make whole" (p. 165). He presents excellent brief discussions of some arcane Hegelian complexities such as absolute knowledge. He properly focuses on rights and the modern state as two central contributions of Hegel's political thought. He relates that thought to important figures from Aristotle and Descartes to Habermas and Gadamer. Smith is particularly sensitive to Aristotle, attentive to Hegel's appropriation (and modification) of key Aristotelian positions, stressing, for instance, the practical reason inherent in Hegel's political system.

In addition, Smith makes two important contributions for Hegel scholars. Smith discusses the right to recognition—courageously undertaking what some foremost Hegel scholars of the previous generation explicitly or implicitly warned against, namely, focusing on the mutual recognition that stems from the master-slave struggle—and discusses it well. As Smith sees it, the right to recognition is a central basis and goal of Hegel's political philosophy, animating his thought.

Smith also clearly presents Hegel's idea of a

critical theory: critique involves the immanent criticism of received opinions, where the skeptical or critical result is taken as "determinate negation" from which a constructive "synthesis" and "reconciliation" can be drawn. Unfortunately, here Smith does not push his analysis far enough. He is unable to adduce Hegel's reasoning that justifies the stage of synthesis and so ends up admitting that "it is not clear . . . why this reconciliation is likely to occur at all" other than "arguably" because of "wishful thinking" (p. 192). Also, since he barely mentions the *Philosophy of Right* (in one of that chapter's 68 notes), its relation to Hegel's critical theory remains unclear.

As a discussion of Hegel's critique of liberalism, Smith's generally sensitive interpretation has, I think, two major problems. While Smith accurately states that "Hegel's views on the state and political community" are his "chief accomplishment" (p. 233), I think that his presentation of Hegel's state as a *Rechtsstaat*—characterized by the rule of law, a disinterested civil service, and a kind of liberal corporatism—does not adequately appreciate Hegel's further accomplishments of insisting that the state be the focus of the citizens' thoughtful patriotism (i.e., that the modern state depends upon its citizens' attitudes for its legitimacy and existence) and that citizens participate in politics (limited, constitutional, and non-democratic though their participation is). In a few scattered sentences Smith does suggest these further dimensions but usually without much emphasis. For instance, recognizing that for Hegel truth depends on dialogue and communication in a community (p. 222), Smith does not then relate Hegel's intersubjective communicative community to his state.

The second problem has to do with how far-reaching Hegel's critique is. Smith plausibly connects liberalism with Cartesian modernity's quest to "master and possess" nature, both external nature and our own passions. Smith states that Hegel's rational freedom also requires that we see ourselves as "separate from and sovereign over nature," both external and internal (p. 211, also 222). If Smith is correct, Hegel seems to be caught up in a modern, Cartesian-Kantian dualism estranged from nature and to be unable to complete his critique of the modern, liberal, divided self despite his stance in his early writings (quoted, p. 53). But, if Smith's word processor, at-

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tempting to emulate *HAL* in 2001, has on its own sneaked in the language of modernity illegitimately, the constitution and unification of the self becomes a central issue that needs more concerted and consistent attention than Smith gives it.

These problems should not, however, obscure Smith's fine treatment of other central issues. Overall, his book is an excellent guide

to Hegel's views of "rights in context" and of the modern state, to their location in Hegel's complete political thought, and to the contexts of contemporary thought into which we should put them.

PETER G. STILLMAN

Vassar College

AMERICAN POLITICS

American Federalism and Public Policy: How the System Works. By Thomas Anton. Philadelphia: Temple University Press, 1989. 244p. \$34.95.

New Federalism: Intergovernmental Reform from Nixon to Reagan. By Timothy Conlan. Washington: Brookings Institution, 1988. 274p. \$34.95 cloth, \$15.95 paper.

The last few decades have seen many changes in the U.S. federal system and not a few efforts at systematic reform. Yet the intergovernmental pattern retains critics aplenty, while the reform approaches have typically achieved far less than intended. These two useful, clearly written volumes contribute substantially to the scholarly dialogue about such matters. Indeed, both do so by exploring the connections between federal arrangements and policy results. And although the approaches undertaken in the books differ, on the whole each is rather successful.

Anton's aim is to scrutinize the U.S. federal system at a general level rather than to conduct another of the innumerable case studies and heavily descriptive surveys so readily available in the intergovernmental literature. He seeks "to develop a conceptually coherent analysis of the relationship between federalism and public policy in the United States" (p. v) by looking for "patterns" amid the bewildering diversity. The tool he utilizes is a "benefits coalition" framework. From this perspective the basic features of the federal structure help to determine the types of coalitions that form policy and the types that shape it: both horizontal and vertical and inevitably involving

governmental officials as well as private actors, the former being linked in "permanently unstable" relationships within a structure of shared authority and political autonomy (p. 231).

The approach draws explicitly from, and seeks to systematize, Monypenny's pioneering work during the preceding generation on the political functions served by federal grant programs. It could also be linked to more recent theoretical efforts on the relationship between policy types and political patterns, although this connection is not developed. There is a bit of the straw man in Anton's repeated reminder that one cannot understand the development of policy in the federal system merely by assuming that policy makers seek to achieve worthy public ends. That "beneficiaries are as important as purposes" (p. 232) is hardly a surprise to any politically astute observer. Generally, however, he does not claim too much on behalf of the approach. The result is a conceptualization that allows a documentation of patterns in such diverse topics as the distribution of benefits, the involvement of private interests in federal programs, and the character of intergovernmental regulation. In one of the most useful sections, he analyzes patterns of intergovernmental finance and offers plausible, if Machiavellian, explanations for the complex and impenetrable revenue-raising arrangements so common today.

Conlan, too, is interested in identifying patterns. But instead of seeking a framework to subsume the diversity of the federal system, his analytic device is a comparison of the two "New Federalisms": the reform efforts of presi-

dents Nixon and Reagan. Conlan's argument, which also relies on a connection between the federal structure and the substance of policies, concludes that although the two reform strategies bear superficial similarities to each other (for instance, each was pursued by a popular Republican president with a stated interest in revitalizing state and local roles in the system), both the intents and the outcomes are markedly different in the two cases. Nixon sought to rationalize the intergovernmental system and to render the structure overall more amenable to governmental activism. Reagan, Conlan convincingly argues, fundamentally sought to shrink government overall both now and for the future. Thus, his New Federalism goals typically gave way to a somewhat conflicting agenda of budget cuts, assistance for business, and reduced government capacity.

Conlan's analysis is embroidered with descriptive detail—perhaps overly so. But the elaborate discussions constitute more than simple intergovernmental history; for the comparison is enriched by a broader theme that is also congenial to Anton's approach, namely, that reform efforts do not take place in a vacuum but derive from particular political perspectives and, in turn, are affected by the environment comprising the policy-making process at a specific juncture. Thus, Conlan finds some of the root causes for the overall failure of Nixon's efforts to derive from the tremendously fractionated policy system of the earlier decade, while Reagan benefited from a "partisan-conservative phalanx" (p. 114) that reached its apex of influence near the beginning of his term. Conlan finds plenty of room for irony in the results: "Paradoxically, the activist and nationally oriented Nixon was overwhelmed by the centrifugal parochialism and fragmentation of the 1970s. In contrast, Reagan, the antigovernment crusader, won crucial victories through his mastery of national politics" (p. 225). In a further twist, Reagan's achievements may have left a bedrock of support for "governmental activism" that can strengthen the welfare state for the longer haul (p. 227).

Each of these carefully constructed arguments, by seeking to discern pattern and explanation after an era of unusually active efforts to reshape the U.S. federal system, enriches our understanding of the subtleties

and dynamics of the system. They are particularly welcome contributions in a field that has been long on description and diversity but short on analysis and grounded generalization. It is thus no real criticism to observe that while both authors find the current setting a propitious one for examining the prospects for systematic revitalization, their conclusions are hedged and somewhat ambivalent. Anton offers very guarded optimism on the grounds that "a broadly based coalition [on behalf of reform] appears to have emerged that is significantly less fragile and less partisan than earlier presidential coalitions" (p. 227); while Conlan—finding the system at a crossroads after the Reagan era—offers two scenarios: an optimistic variant involving state renaissance but also a bleak sketch of "co-optive" federalism with the national government increasingly in control of state and local actions (p. 231).

These volumes are solid, explicitly political analyses and convey complementary insights. Anton elaborates a general conceptual approach that offers promise as a way of identifying pattern and regularity amid a welter of governments and programs. Conlan, by comparing different reform efforts and linking these to dynamic features of the policy-making system, sensitizes the reader to the importance of a changing political context. Novice students of the federal system and seasoned scholars alike can gain plenty from the perspectives offered here.

LAURENCE J. O'TOOLE, JR.

Auburn University

Managing Uncertainty in the House of Representatives: Adaptation and Innovation in Special Rules. By Stanley Bach and Steven S. Smith. Washington: Brookings Institution, 1989. 140p. \$22.95 cloth, \$8.95 paper.

The uncertainty of floor decision making and the use of special rules to structure floor debate on major legislation in the House of Representatives is the topic of *Managing Uncertainty in the House of Representatives*. Stanley Bach and Steven Smith know the House and its doings as well as any of its students. Their book is readable and accurate. It is a must for watchers of the legislative processes of the House. The special rules Bach and

Smith describe and analyze play a crucial role in the present processes of the House. The importance of the special rules match the changes in the House Rules Committee. Had there not been dramatic changes in the Rules Committee itself, the special rules could not have become a major part of the process of integrating the floor action in the House on important and controversial legislation.

Bach and Smith emphasize the 1961 "packing" of the Rules Committee, which made possible the nominal capture of the committee by Democratic-oriented leadership majority. Other events over a five-year period were all vital to the ability of the Rules Committee and the Democratic leadership to craft and to use special rules. These included the defeat in a primary in the mid-sixties of Howard Smith of Virginia, the enormously able and reactionary chairman of the Rules Committee, who regularly and successfully defied the legislative desires of the now-mythic Speaker of the House, Sam Rayburn. Smith was succeeded as chairman by a less-able and less-fanatic conservative, whose influence was less distinctive of the desires of the majority of Democrats and their leaders. In the mid-1970s the ratio of Democrats to Republicans was increased from a majority of one to the much safer margin of two to one plus one. That assured under all but the most peculiar circumstances a real majority supporting the Democratic leadership rather than a nominal one that sometimes was overcome by the continuing alliance of conservative Republicans and conservative Democrats, whose coalition dominated the House committee system during much of the last 40 years. That majority and the Democratic leadership actively sought methods in and out of the committee itself to make possible successful procedural outcomes in intricate and complicated legislative situations. Many of the new problems were the direct result of the outmoded organization of the House committee system and the increased complexity of the legislative process after the enactment of the Budget and Impoundment Control Act of 1974.

Fortunately those new problems were matched, at least to a degree, by the new powers provided to the Democratic Speakers, a variety of procedures that gave them greater influence over not only the Rules Committee but also the committee system generally.

Those included the creation of a new Steering and Policy Committee in the Democratic caucus, which was chaired by the Speaker and took over most of the work of selecting Democratic nominees for committee assignments. The Democratic Speaker's exclusive power to nominate Democratic members of the Rules Committee, while not so draconian as it is taken to be, did ensure that fanatic opponents of Democratic policies who masqueraded as Democratic members of the House could no longer clog up the legislative process all the time.

Subsequent Speakers—whether good legislators themselves or not—had spectacularly good staffers who were students of the legislative process. The most outstanding of these were John Barriere, who served both Speaker John McCormick and Speaker Carl Albert, and Ari Weiss, who was an invaluable aide to Speaker Tip O'Neill. Neither is well known, but each made contributions to the legislative process at least as large as the great majority of members. One of those Speakers was himself a highly intelligent and skilled legislator who understood the processes of the House very well. Speaker Carl Albert was quietly responsible for much of the success of the efforts to modernize and even reform the House and its committee system.

Within that framework it was possible for the Rules Committee and the Democratic leadership to manage uncertainty in the legislative process at least in part by the use of the special rules that Bach and Smith so well describe in this particularly useful book.

RICHARD BOLLING

Race, Class, and Conservatism. By Thomas D. Boston. Boston: Unwin Hyman, 1988. 172p. \$34.95 cloth, \$11.95 paper.

Conservative Mythology and Public Policy in America. By Arnold Vedlitz. New York: Praeger, 1988. 173p. \$35.95.

Both these books offer a critique of conservative perspectives on public policy. *Race, Class, and Conservatism* presents a detailed response to the recent writings of William Wilson, Thomas Sowell, George Gilder, and Walter Williams. Its central thesis is that contrary to the conclusions of these social scien-

tists, racial discrimination continues to play a central role in shaping the life chances of black citizens. Vedlitz's book, although somewhat broader in scope, also focuses extensively on issues of race and poverty. *Conservative Mythology and Public Policy in America* seeks to refute a number of the "myths" that underlie various conservative public policy prescriptions. Boston's study is the more scholarly of the two, while the work by Vedlitz appears to be written more for use as a text.

Both authors devote a disproportionate amount of attention to criticizing the work of Thomas Sowell. Boston acknowledges the validity of Sowell's contention that the differences in earnings between blacks and whites diminish when one holds constant such factors as education, age, geographical location, and job experience but goes on to argue that wage inequality constitutes only one form of discrimination, the different rates of employment for blacks and whites being equally important, although Sowell has evidently overlooked it. Boston also contends that many of the variables that Sowell holds constant are themselves due to racial discrimination, thus making Sowell's analysis tautological.

Vedlitz, in turn, challenges Sowell's position that the experiences of blacks in U.S. society can be usefully compared to that of other ethnic or racial minorities. He contends that no other group in the United States has experienced comparable levels of discrimination and that the legacy of the legal restrictions placed on blacks prior to 1965 remains critical to any understanding of their contemporary predicament. Both Vedlitz and Boston take particular pains to refute Sowell's contention that the superior performance of West Indian blacks constitutes evidence that cultural values rather than racial discrimination explain the relatively small size of the black middle class.

Both books present sophisticated analyses of the debate over the causes of the racial inequality in the United States. They demonstrate an impressive command of the relevant literature, and their data analysis is both clever and original. In particular, their exhaustive critiques of Sowell's methodology deserve to be taken seriously by any scholar working in this field. Hopefully, these books will stimulate additional debate.

Yet at the same time, both studies are disappointing. For all their sophisticated data

analysis, they never really seem to come to grips with central insights of the revisionist literature on race and economics. In the final analysis, their research suffers from the same sort of ideological blinders that they attribute to those scholars with whom they disagree.

Consider, for example, their treatment of the "culture-of-poverty" thesis. Boston dismisses those who believe in the existence of such a culture with a footnote while Vedlitz takes a whole chapter to demonstrate that the culture-of-poverty thesis is "without merit" (p. 34). Yet, significantly, the words *crime* and *drugs* do not appear in either book. Neither author ever acknowledges the dramatic *deterioration* in the condition of a large number of urban blacks over the last two decades—a tragedy reflected (though far from fully captured) in such indexes as the decline in the rate of young male labor force participation and the growth in the number of black children raised by one parent or by no parent.

The persistence of a black underclass could plausibly be attributed to both past and present discrimination. But in the absence of any evidence that racial discrimination has actually *increased* since 1965, other explanations need to be examined. Vedlitz is wrong to equate the culture-of-poverty argument with "blaming the victim." On the contrary, as students of political culture have long understood, a group's culture or subculture is the product of a complex array of economic, institutional, and historical factors—many, though not all, beyond its control. Although the persistence of racism is not irrelevant to understanding the growth of a black underclass, neither does it fully explain it.

More importantly, attributing the problems of black citizens to racial discrimination does little to assist policy makers in designing more effective programs to reduce racial inequality. Both authors clearly understand that the ideas of social scientists who write about race in the United States have important political implications. Indeed, that is precisely why both these books were written: their common aim is to undermine the credibility of scholars who assert that racial discrimination has diminished in importance. The explicit purpose of this exercise is a political one—to challenge the intellectual underpinnings of conservative political ideas and programs.

Yet while both authors mock the failures of

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supply-side economics, these books are curiously devoid of any policy prescriptions. What policy prescriptions flow from *their* intellectual premises? How, for example, should we encourage more black teenagers to graduate from high school or promote black-owned businesses? How can we promote black political leadership and participation, particularly at the neighborhood level? Moreover, if Charles Murray's work is flawed, does that mean that we should automatically dismiss *all* conservative policy proposals, such as tenant ownership or management of public housing and school vouchers? Are none worthy of being explored?

Finally, Boston's book suffers from an inadequate analysis of the internal political dynamics of the black community. He dismisses the political importance of conservative black scholars such as Sowell by stating that their influence is solely a function of the political strength of white conservatives. That may well be true. But then he ignores the arguments of those who contend that the civil rights "establishment" is also out of touch with the concerns of large numbers of poor blacks. In any event, this is not a subject for polemics but for survey research.

DAVID VOGEL

University of California, Berkeley

Corporatism and the Rule of Law: A Study of the National Recovery Administration. By Donald R. Brand. Ithaca: Cornell University Press, 1988. 340p. \$39.95 cloth, \$12.95 paper.

Class Struggle and the New Deal: Industrial Labor, Industrial Capital, and the State. By Rhonda F. Levine. Lawrence: University of Kansas Press, 1988. 233p. \$25.00.

These two works offer widely divergent approaches to, and interpretations of, the political significance of the New Deal. Rhonda Levine, writing from the perspective of Marxist sociology (specifically, the variant of Marxist theory espoused by Nicos Poulantzas), adopts a "class-centered approach" based on "the Marxist premise that capitalism is a contradictory system." She aims in particular to

correct the "state-centered" interpretation of the New Deal set forth by Theda Skocpol, among others, which Levine believes "ignore[s] the importance of the underlying economic processes that shaped the structures of the state in the first place" (pp. 10-13). Donald Brand focuses on the National Recovery Administration (NRA) as a test of Theodore Lowi's thesis that the rule of law has been undermined in the twentieth-century United States by the rise of "interest group liberalism" (corporatism), a system that by enhancing the discretion of administrative authorities in interpreting the law allows private groups to "capture" public authority so as to serve their own ends, to the detriment of both equity and efficiency (pp. 3-5, 12).

Levine views the New Deal as a period in which governmental structures and policies were reorganized so as to promote "a renewal of capital accumulation," adapting to the new dominance of "monopoly capital" in the accumulation process (p. 172). She notes, however, that the "conspiracy" model adopted by those who treat the state as merely the instrument by which the economically dominant classes advance their interests "oversimplifies the nature of class relations" and fails, at the same time, to account for capitalist resistance to the very New Deal policies that ultimately served their interests (p. 6). Taking account of the fact that the capitalist state "is not a rational organization . . . that merely reflects economic class relations," she undertakes to demonstrate how "the exigencies of the balance of class forces and uninterrupted accumulation of capital" nonetheless "limited political decision making and the formulation and implementation of New Deal policies" (pp. 14, 19).

While Levine's work is heavily footnoted and includes references to such primary sources as the Roosevelt papers and the files of various government and labor organizations, the primary materials are chiefly illustrative rather than demonstrative of her argument, the bulk of which relies on prior secondary treatments of the New Deal, typically from a Marxist orientation. Her book takes the form of a historical narrative, examining the NRA, the debate over antitrust policy, and the struggle for union recognition. Yet it is questionable whether the narrative supports her theses or even whether the theses are capable of confir-

mation (or disconfirmation). Typically, her argument consists of assertions (notwithstanding the disclaimer quoted above) that particular government policies reflect "the balance of class forces" (pp. 65, 69, 71, 80, 131) or mirror "the larger antagonisms and contradictions of class society" (p. 88). But since she devotes little attention to the actual process of policy formulation and development within the government, she can hardly be said to demonstrate that the Roosevelt administration was primarily reflecting, rather than shaping, "the balance of class forces." She cites a single entry from Rexford Tugwell's diary prior to Roosevelt's inauguration saying that "FDR does not realize what the issues are" to demonstrate that "the entire New Deal program was not clearly thought out by anyone, least of all by Roosevelt himself" (p. 68).

Aside from her failure to undertake a serious analysis of Roosevelt's statesmanship, Levine's argument has the appearance of being written in a historical or comparative vacuum. She contrasts "the victory of the workers' revolution in Russia" with the "political repression" of workers and "radicals" in the United States (p. 38), disregarding much vaster Soviet repression and massacres that followed the 1917 revolution and continued through, and beyond, the New Deal period. She cites the 47% rise of real wages in the United States during the 1920s as evidence of "a steady increase in the rate of exploitation" because "real value added per worker rose 75% for the same period" (p. 21) and deprecates this rise as slight or small (pp. 30, 45, 48)—as if citizens of the "workers'" state would not have envied such "exploitation"! She approvingly cites another scholar's interpretation of the U.S. military buildup begun in 1938 as reflecting "American imperialism" (p. 150) and laments the postwar "purging" of communists from trade unions and the Democratic party (p. 157), without any reference to the world situation that generated these policies. Overall, this is more an ideological document than an original contribution to scholarship.

Despite its seemingly narrower theme, Brand's book makes a far broader theoretical, as well as historical, contribution. Without denying the confusion of goals that sometimes characterized New Deal policies, he traces the intellectual roots of the NRA to the Progressive movement, several of whose leading mem-

bers favored a "corporatist" approach to economic management. Progressivism shaped the beliefs of the leading drafters and administrators of the NRA, including Raymond Moley, Donald Richberg, and Tugwell. In contrast to the claims of such revisionist historians as Ellis Hawley that the NRA, like the New Deal generally, favored the interests of big business (claims reflected in Levine's assertion that "the NRA was dominated by monopoly capital; it was made impermeable to all but monopoly interests" [p. 80]), Brand documents considerable big business opposition to the program, as well as the fact that the program's operation typically favored the interests of smaller businesses at the expense of their larger competitors. The core of business support for the extension of the NRA, he emphasizes, came from "peripheral" firms—businesses that typically are labor-intensive, lack vertical integration, and are unable to develop "oligopolistic power over their market" (pp. 146–47). Brand supports his thesis with case studies of the oil and steel industries, the former because it exemplified the conflict between the interests of large and peripheral firms, the latter because it "represents the relatively anomalous case of an oligopolistic industry supporting the NRA, albeit with considerable reservations." He explains the apparent anomaly by the fact that for steel companies the NRA constituted a relaxation rather than an extension of "the regulatory state," through its mitigation of previous antitrust restrictions on typical industry practices. Even in this case, however, "the initial NRA emphasis on industrial self-government increasingly gave way to a more regulatory posture as it became evident that industries were using the NRA to cartelize" (p. 225). Finally, Brand argues that "the NRA was far more sympathetic to labor" than pluralist theorists such as James MacGregor Burns or "progressive" historians like Arthur Schlesinger have acknowledged (pp. 23–24, 30, 288–89, 303). In contrast to Burns's treatment of Roosevelt as a "broker politician" (and Levine's dismissal of the entire Roosevelt administration as an epiphenomenon of shifting class forces), Brand emphasizes FDR's consistent commitment to progressive goals and his determination to subject business to regulation on behalf of the public interest, dating at least as far back as his governorship of New York. While flexible in his choice of means and com-

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pelled, in the case of the NRA in particular, to make concessions to business as well as labor interests for the sake of the voluntary cooperation the program was intended to engender, Roosevelt remained "in the driver's seat when it came to the legislative process" (p. 268). The "systematically pro-labor policy" of the NRA is attested not only by the considerable growth in union membership during its operation (a fact acknowledged by Levine) but also by the consistent support given to the NRA by John L. Lewis, the militant leader of the United Mine Workers.

In his overall evaluation of the NRA, Brand agrees with previous analysts that the program failed to achieve the progressive goals in the name of which it had been enacted; but he denies that the failure was due to the "capture" of the regulatory authorities by oligarchic interests or that the NRA's "corporatist" approach is exemplary of an oligarchic bias typical of modern U.S. government, as Lowi's thesis would suggest. The NRA's failure is attributable rather to its advocates' unrealistic goal of fashioning "a societal corporatist peace treaty" between business and labor (p. 289) combined with the tension *within* the progressive vision of promoting both increased competition and heightened security for individual firms (pp. 66-68, 157-62, 206).

While controversy over the significance of the NRA and the New Deal as a whole is bound to continue, Brand has advanced our understanding of U.S. politics most impressively by meeting both Lowi and contemporary pluralist theorists on their own ground, thereby demonstrating the continuing relevance of the Founders' understanding of, and remedy for, the problem of faction. Rejecting the "sharp dichotomy of a government of laws or a government of men . . . that Lowi accepts" (p. 299), the Founders endeavored to establish a system that would secure individual rights against arbitrary majority or minority factions while still allowing the government (especially the president) the discretion necessary to adapt differing means to the fixed ends specified in the Declaration of Independence. The real deficiency of NRA administration, Brand suggests, arose from the pragmatist-Progressive overreaction to the exaggerated formalism of pre-1937 laissez-faire jurisprudence—a a formalism paralleled (albeit in the name of more egalitarian ends) by Lowi's pro-

posed "juridical democracy." Not only political scientists interested in the New Deal period but all those seeking a better understanding of the contemporary problems of liberal constitutionalism will learn much from Brand's study. It embodies an exemplary combination of meticulous empirical study with theoretical depth.

DAVID LEWIS SCHAEFER

Holy Cross College

Congressional Politics. Edited by Christopher J. Deering. Chicago: Dorsey, 1989. 337p. \$18.00.

Literature on the United States Congress abounds. Thus, in order to make a new work viable, the author must present the institution in some new light. Christopher Deering has set out to do this by setting the essays in *Congressional Politics* within a framework assessing Congress from three "levels of analysis."

Deering contends that the workings of Congress are best understood through examining its operations at three levels. The first takes the whole as the sum of its parts; the second views Congress as an institution with its own folkways shaping the behavior of Congressmen and the legislation they produce; and the third perceives an institution influenced by the political environment.

Using contemporary cases, the contributors demonstrate how Congress makes policy in the light of constraints and structures faced at each level of analysis. The various chapters demonstrate how different types of legislation will be more or less general, depending on the level at which Congress operates.

Deering introduces his approach with a device enabling the reader to get to the heart of the concept. Citing the development of three provisions of the 1986 Tax Reform Act, Deering demonstrates how each level influenced the statute. Similar anecdotal devices introduce the central precepts in many of the contributions to the book. This, along with the liberal use of contemporary examples and simple charts and tables, makes the arguments proposed throughout the book easily accessible.

The book examines the workings of both chambers, including Oleszek's informative chapter on legislative procedures, and the rela-

tionship between House and Senate. The various chapters show how the two institutions complement each other in a way that adds a further dimension to the sharing of power envisioned by the Founding Fathers.

Many of the chapters in the book are adapted from articles presented elsewhere or are distillations of major research on the subject, rather than original research. Although the book presents little in the way of new findings, *Congressional Politics* does offer some provocative insights regarding how the processes of the institution may be used for particularistic purposes.

In bringing together a collection on Congress, Deering goes up against some formidable opposition, namely Dodd and Oppenheimer's *Congress Reconsidered* (1989), now in its fourth edition. Dodd and Oppenheimer's work is painted on a broader canvas than Deering's, focusing on the electoral dimension and congressional change. *Congressional Politics* is more static and in some senses more elementary, and addresses only one (albeit large) dimension of the institution. While Deering's collection may produce less in the way of new information than the book to which it will inevitably be compared, however, it does propose a different way of examining policy outputs, not a significant concern of *Congress Reconsidered*.

Inevitably there are pieces of the Congress puzzle that are omitted from Deering's book. This is in part due to the fact that Deering was not trying to create a textbook but to demonstrate the diversity of influences upon congressional outputs. At the same time, some of the influences addressed may be more important than the writers would have us believe. Tierney and Schlozman, for example, seem to contend that the power of organized interests is largely the creation of overzealous journalists and ignore the role that soft money plays in electoral politics and congressional decision making.

The key question to ask in evaluating Deering's collection is whether we learn more about Congress by looking at it from an integration of these perspectives. The structure does provide a frame of reference demonstrating the diversity of influences on policy making. Many of the chapters also underline the importance of maintaining a healthy skepticism regarding book perspectives that present Con-

gress as a deliberative body composed of rational individuals with pure, democratic motives.

It may not always be, however, that a policy output has emerged from a single "level." This makes the organization of the book a little precarious. For example, Hurley's chapter on parties and coalitions is set within the context of the institutional Congress; yet she concludes that external factors, such as the president's partisanship, are persistent influences on floor voting. This does demonstrate how each level of influence is interrelated, but the book as a whole contains no conclusions assessing the relative importance of each level or the impact of the various influences on the democratic process.

Congressional Politics, nonetheless, does contribute to the literature by prompting the reader to consider the diversity and interrelation of influences on Congressional outputs.

MAUREEN P. GARELICK

University of California, Santa Barbara

Advice and Consent: The Development of the Policy Sciences. By Peter deLeon. New York: Russell Sage Foundation, 1989. 143p. \$17.50.

Social Science in Government: Uses and Misuses. By Richard P. Nathan. New York: Basic Books, 1988. 240p. \$18.95.

These are two interesting books on essentially the same subject. What Nathan calls "applied social science" largely corresponds to what deLeon calls "the policy sciences." Both authors take stock of the field, present histories of how it developed to where it now is, and provide critical evaluations of the current state of affairs. Their complaints are generally about the same things. There is broad agreement, too, regarding where the applied social sciences (policy sciences) should go, but also some important differences.

The similarities in viewpoint are not surprising. Both authors are political scientists by training, and both display a certain righteous resentment at what they consider to be the undue influence that economists have played in

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the field. Both have served as practitioners as well as scholars and thus are able to look at the policy making—social science interaction from both sides of the street. And both strongly desire the social sciences (policy sciences) to play a stronger and more salutary role in guiding social problem solving.

Richard Nathan has a relatively clear proposal, and it is a radical one. He calls for a significant shift in the allocation of resources in the applied social sciences toward studies of "how to do it." In a sense he is calling for applied social scientists to operate less as basic scientists and more as social engineers, and he presents a description of what that entails.

He sees several things wrong with the current orientation of applied social science. There is too much apeing of natural science, he says. Nathan says he believes the hunt for simple formal mathematical models that tell us much is doomed to failure. He also argues that the social scientists' love for quantification and disdain for qualitative characterization is a mistake. He sees social problems and appropriate policy responses as complex and argues that dividing the intellectual turf by disciplines is an inadequate way to see the real issues whole.

Nathan posits that doing good applied social science requires that social scientists get deeply immersed in the problem being studied, as contrasted with the more antiseptic and piecemeal approaches currently in vogue. More specifically, he advocates that a significantly enlarged share of the effort of social scientists be focused on studying policies in action to find out what works, how they work, and how they can be made to work better. To have maximal impact on policy (and to attract the significant funding that the kinds of study he advocates entail) he proposes that these efforts be focused on issues in which policy makers are now genuinely interested but where there is some patience to wait for answers.

Nathan has interesting things to say about how to go about good studies of policies in action. His argument for the use of control groups in policy experiments is persuasive. His description and analysis of a number of policy studies in which he has been involved, in one way or another, is enlightening. However, the basic flavor of the book is set by the advocacy of the point of view described above.

Since the book is addressed to government

research funders, and foundations, as well as to social scientists, Nathan's views may end up having teeth. If so, for better or for worse, the applied social sciences will be different.

DeLeon's essay is more general and broad, less specific and pointed in the guidance he wants to provide. He has many of the same complaints as does Nathan about the current state of "policy science." He clearly does not like the way most economists view the field. He, too, argues for more attention to qualitative phenomena and variables and to interdisciplinary approaches.

However, if I read him right, he probably would be much more reserved than Nathan about concentrating the lion's share of attention on what politicians and high-level civil servants are interested in today. He stresses, much more than does Nathan, the role of the "policy sciences" in identifying and characterizing social problems and in being ahead of the policy action, indeed, ahead of what policy makers and laypersons are even thinking about in a concrete manner. I confess having considerable sympathy for deLeon's view on this. For someone who has this point of view, Nathan's proposal that a significantly larger share of the overall research effort be focused on current perceived policy problems and options would seem something of a threat.

Certainly, deLeon articulates much more sharply than Nathan the position that the policy analysis community ought to have a certain intellectual independence, that it not become the servant of policy makers, that policy analysts avoid being "hired guns."

Indeed, deLeon picks up on a proposition that C. E. Lindblom has been making, namely, that the audience of policy analysts ought to be as much citizens as politicians and government officials. While Nathan sees what applied science should be doing as "social engineering," deLeon sees the role more as that of providing the "intelligence of democracy."

All in all, I liked both books. Both will be useful additions to policy analysts' (applied social scientists') bookshelves. Both, or portions of both, would make useful parts of reading lists for courses in policy analysis (applied social science). To recapitulate, one striking feature of these two books is how much they agree on a variety of points. But another striking feature is the difference in the visions presented of the appropriate roles of the ap-

plied social sciences, or policy sciences, in democratic political process.

RICHARD NELSON

Columbia University

The American Constitutional Tradition. By Daniel J. Elazar. Lincoln: University of Nebraska Press, 1988. 291p. \$32.50.

This book is about federalism and not about the U.S. constitutional tradition as a whole, as its ambitious title might suggest. Federalism, however, in the capable hands of Daniel Elazar, is not confined to the intricacies of intergovernmental relations. In the introductory chapter, the author declares his intention of examining federalism in its etymological and biblical sense as a covenant. This declaration launches a wide-ranging, informed discussion of liberty, civic virtue, pluralism, religion, and a host of other topics that climax in a powerful and provocative closing chapter on "exceptional situations" that have legitimately called for actions that violate constitutional norms. Thus, Elazar's broad understanding of federalism redeems the grandiose title he has chosen.

The volume is a collection of essays written at different times for different occasions and therefore, like most undertakings of this nature, fails to develop a sustained argument with a clear beginning, middle, and end. An excellent preface and introduction, however, give the reader a good overview of how the various chapters are related; and this, in turn, enables the reader to discern significant and impressive themes throughout the book.

The most important of these themes is federalism as covenant, which Elazar carefully defines as follows: "A covenant is a morally informed agreement or pact based upon voluntary consent and mutual oaths or promises witnessed by the relevant higher authority, between peoples or parties having independent, though not necessarily equal status, that provides for joint action or obligation to achieve defined ends (limited or comprehensive) under conditions of mutual respect that protect the individual integrities of all the parties to it" (p. 90).

Working from this definition of *covenant*, Elazar draws a sharp distinction between the

"natural liberty" of the social contractarians' state of nature on the one hand and the "federal liberty" of John Winthrop on the other. By federal liberty Winthrop meant moral liberty, which is liberty "to that only which is good, just, and honest." Elazar readily concedes that Winthrop's position "reflects a republican predemocratic understanding of the political order." Nevertheless, he maintains, "we can still draw upon its essence to understand the federal liberty of the individual in a democratic civil society" (p. 167).

Federal liberty provides the moral foundation for the political order known as a commonwealth, that is, "a state in which the whole people have an individual interest" (p. 172). The commonwealth aspect of the U.S. political tradition is found primarily in the early state constitutions, especially in the New England states. Elazar contrasts the commonwealth model with the self-interested "marketplace" model of Publius and his soul mates down to the present day. Thus, Elazar's discussion of federalism escapes the tedious boundary questions of state versus national *powers* and offers instead a rich, normative consideration of the task of federalism as one of striking the appropriate balance between the substantive moral values of the commonwealth tradition, which resides in the state constitutions, and the "rules-of-the-game" approach of the federal constitution that ensures procedural fairness in the economic and national market place.

With this high-toned federalism in mind, Elazar examines contrasting traditions of pluralism among the states themselves and contrasting visions of the most desirable public order as manifested in different constitutional traditions among the states.

Applying his marketplace-versus-commonwealth distinction, he faults the unhappy experiment with prohibition as an ill-advised national effort to create a commonwealth. Likewise, he faults Supreme Court decisions that have restricted state initiatives in such areas as "the relation between religion and state, obscenity and pornography, abortion, Sunday closings, and required days of rest, to name a few of the major issues" (pp. 175-76). Elazar disapproves of these decisions because they deny the states their traditional powers over "transcendent moral issues." Through its decisions in these matters, "the Court has consistently interpreted the federal

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constitution in such a way as to extend the market place into the commonwealths" (p. 175).

With his fresh understanding of federalism, Elazar's essays provide a useful addition to the growing body of constitutional literature that addresses the dangers of an excessive individualism in the United States today. By highlighting the commonwealth aspect of our constitutional tradition, he makes it easier to criticize excessive individualism from within the constitutional tradition.

JOHN A. ROHR

Virginia Polytechnic Institute

Wage Justice: Comparable Worth and the Paradox of Technocratic Reform. By Sara M. Evans and Barbara J. Nelson. Chicago: University of Chicago Press, 1989. 224p. \$24.95.

Equity and Gender: The Comparable Worth Debate. By Ellen Frankel Paul. New Brunswick: Transaction, 1989. 143p. \$22.95.

In Leviticus 27:3-4 it is written that "when anyone explicitly vows to the Lord the equivalent for a human being, the following scale shall apply: If it is a male from twenty to sixty years of age, the equivalent is fifty shekels of silver by statutory weight; if it is a female, the equivalent is thirty shekels."

Certainly, the notion of differential worth is not new to the United States, as it is deeply rooted in Judeo-Christian tradition. Though the ratio of "worth" has changed in the past several years, the principle of differential worth of the jobs performed by women as opposed to those performed by men remains a reality. This condition, as well as the appropriateness of altering this situation, are the questions that the authors of two new books on comparable worth address.

Most women who work out of their homes are clustered in the low-paying, "pink collar" ghetto. These women workers perform many of the most important jobs in the economy. They serve as teachers for our children and primary providers of health care in hospitals and nursing homes, and they are the backbone of the financial and business office world. In

fact, over half of all women employed outside of their own homes work in fields that are 75% dominated by women. At the present time it is estimated that the more a job category is dominated by women, the less it pays. Eliminating the inequities associated with this condition often requires that a reevaluation of all jobs be accomplished on the basis of the skills and responsibilities that they require. Though there have been some adjustments made during the past decade, it is instructive to consider the pay scale for animal health technicians and nursing assistants in the federal government in the early 1980s. Animal health technicians, of whom 94% were male, earned an average of \$19,340 per year; whereas nursing assistants, 67% of whom were female, earned an average of \$13,890 per year.

Most of the attention of comparable worth advocates has been focused on the public sector and more specifically on state and local governments. Much of the conventional wisdom on the subject assumes that if governments initiate pay equity systems, they will serve as models for private sector concerns to follow. Also, governments are often among the largest employers in a given area. Inasmuch as the focus of attention has been the states and local governments, it is not surprising that the authors of both *Wage Justice* and *Equity and Gender* chose to consider the issues of pay equity in state settings.

Evans and Nelson use extensive data from Minnesota, a state in which pay equity has proceeded more rapidly than in other states, and consider the implementation of public policy. They provide a thorough analysis of the stages of problem definition, policy initiation, and political decision making against a detailed backdrop of historical events. The study is based in large measure on a survey of state employees and on in-depth interviews with the primary actors in the quest for pay equity for public employees in the over-sixteen hundred local jurisdictions and the state. On the basis of the data that they collected, as well as on very astute observation and analysis, Evans and Nelson concluded that "comparable worth cannot bear the full weight of proponents' hopes for democratic change and wage justice." Though comparable worth legislation certainly improves conditions for women, it is a "procedural reform"; as such, it does not create new individual rights. Rather,

they argue, the legislation must be seen a part of a larger strategy linked to other concerns.

Whereas Evans and Nelson believe that the absence of comparable worth is an absence of equity in a democratic society, Paul, in *Equity and Gender*, argues the pros and cons of pay equity and concludes that "comparable worth . . . depend[s] on some rather dubious assumptions and . . . embrace[s] a view of equality that is at odds with our American tradition" and is "incapable of being put into practice without chaotic results." What Paul is advocating in this volume is a maintenance of the market system of determining wage rates because she believes that that system works well, that it undergirds a system of equality of opportunity, and that it is congruent with our tradition of liberal democracy. Moreover, she feels that women have benefited from the market system. She points to the increased number of women in the paid work force and the new employment opportunities that have been opened to women in the past two decades.

In the 29 February 1988 issue of *Business Week* magazine it was noted that women "have made real progress. By 1986 women's earnings had climbed to an average of \$16,232 a year vs. \$25,256 for men." Evans and Nelson would argue that much of this difference can be explained by the pay scales associated with the positions that are predominately female, as opposed to those which are dominated by males. Moreover, they would note that the inequity can and should be reduced by conscious public policy in the form of comparable worth legislation. Paul, on the other hand, feels that this condition is a justifiable outcome of the economic system, which is reasonably fair and which has treated women, at least in the recent past, fairly well.

The two books are very different from each other. Whereas Evans and Nelson treat readers to an insightful glimpse of the policy process, Paul tries to assess both sides of a very emotional issue before she says *no* to comparable worth as a remedy for the inequities in pay between the sexes.

MARIAN LIEF PALLEY

University of Delaware

The Making of a Senator: Dan Quayle. By Richard F. Fenno, Jr. Washington: Congressional Quarterly, 1989. 180p. \$19.95 cloth, \$13.95 paper.

The sudden, unpredictable anointing of Dan Quayle, Indiana's junior senator, as a national political figure was a mixed blessing for Richard Fenno, as for the rest of us. Fenno, in the process of examining Quayle and 16 other senators, found himself the possessor of unique information on the new vice president's formative political years. Yet Quayle's story was only one of many for Fenno, who faced the dilemma of allocating limited research time and budget among his subjects. Now Fenno's vexing dilemma is ours as well: how to assess Quayle's Senate career—its interlocking "seasons" of campaigning and governing—in terms of the suggestive categories the author developed for comparative analysis of office-holders.

Peerless observer that he is, Fenno soon grabbed hold of the big story of Quayle's Senate career: his successful sponsorship of employment and training reauthorization, the Job Training Partnership Act (JTPA) of 1982. Quayle's resources are skillfully detailed: his independent streak, his bubbling self-confidence, his choice of a skilled staff aide (Robert Guttman), and his unexpected alliance with liberal nemesis Edward M. Kennedy. As an ambitious player entering the venerable "manpower subgovernment," Quayle wisely followed a bipartisan course; but he reckoned without the Reagan administration, which had little understanding of, or patience for, substantive policy making, especially employment and training programs. The ineptitude of Reagan's Labor Department was boundless. (Their deputy was so distrusted that he was at first barred from the room where the committee was marking up the bill.)

Quayle leapt into the vacuum created by the Reagan administration and its Labor Department, not to mention the beleaguered Senate committee chair, Orrin G. Hatch (R-Utah). Propelled by mounting unemployment in the 1982 recession, Quayle won overwhelming passage of his bill and maneuvered the White House into embracing it (although he had to sneak in the back door to get a publicity photo with Reagan). It is a pointed lesson in separation of powers and senators' independence

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even from a popular administration they had gone down the line to support.

Even though he highlights this fascinating case, Fenno cannot pursue all the angles—for example, how other players viewed Quayle's JTPA role, much less how Quayle performed on other issues. Events and results are left dangling. If Fenno wasn't there or if Quayle wasn't at the center of the event, we tend not to hear about it.

Quayle and the JTPA allow the author to describe the politician's sequence of activities, from campaigning to governing and back again. Fenno has a chance to apply his "home-style" categories and to show how Capitol Hill activity translates into electoral strength. He is at his best analyzing Quayle's approach to his various constituencies and explaining JTPA's role in establishing Quayle's credentials as an "effective senator." In the face of voter concern about unemployment, the young senator's authorship of a jobs bill inoculated him against attack. His version of the facts, moreover, went unchallenged. Reporters never asked him probing questions, and his inexperienced 1986 opponent failed to challenge or question his account of his Capitol Hill triumphs.

Fenno clearly identified with his beguiling subject; and from the narrative presented here, the reader is inclined to agree with his positive impression. But lacking the insights and perspectives that will no doubt flow from the author's 16 other cases, we cannot compare Quayle's metamorphosis with those of others at similar stages of their careers.

Quayle's later notoriety—not to mention his apparent slowness in adapting to the national spotlight—is not fully foreshadowed by his senate career. The neophyte legislator is the hero of the JTPA story, no doubt about it. His reelection seems equally masterful. But what does Quayle's "individual growth and accomplishment" (another of the author's core themes) tell us about his likely future development as vice president? Not as much as we would like. It is easy to see why Fenno was impressed with his subject. But without comparisons with other aspects of Quayle's career—much less careers of other senators—it is hard to know what to make of Quayle's optimistic demeanor, risk-taking style, and independent streak.

Here, then, is the mixed blessing of Quayle's

notoriety. Standing alone, Quayle's story is fascinating yet incomplete. As one of 17 cases studied, the Quayle saga will presumably fit seamlessly into the rich, subtle picture of Senate careers that Fenno is constructing. Until Fenno is ready to weave the stories of the 17 senators into his promising framework, we will have to content ourselves with this intriguing fragment. And as everyone knows by now, no one else matches Fenno's ability to wring meaning and insight from the observation of politicians practicing their craft.

ROGER H. DAVIDSON

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Fallout: An American Nuclear Tragedy. By Philip L. Fradkin. Tucson: University of Arizona Press, 1989. 301p. \$24.95 cloth, \$14.95 paper.

The United States may never have seen more negligent and obtusely nearsighted government behavior than that having to do with radioactive fallout from nuclear weapons testing in Nevada back in the 1950s. First, at the time of the tests there were the actions and decisions taken and not taken that led to radiation exposure of people and their livestock living downwind. Then, more recently, there have been the decisions refusing to honor even the most solidly based damage claims arising from that earlier history of negligence and disregard.

Philip Fradkin is an angry, impassioned chronicler of this sorry record. First-year medical students are said to sometimes take on symptoms of diseases they are studying, and Fradkin reports a like experience from his immersion in the story of real and alleged cancer victims among the "downwinders" exposed to fallout. "I felt weak and nauseous at times and exhibited other symptoms of that most dreaded disease," he writes.

Fradkin was once a reporter for the *Los Angeles Times* and shared a Pulitzer Prize for that newspaper's coverage of the Watts riot. This book, too, shows evidence of exhaustive journalistic inquiry. The trouble is that Fradkin has such strong likes and dislikes, such a predisposition to evaluate certain personalities and events favorably or unfavorably, and

such a weakness for the non sequitur that this is not the scholarly work that his very considerable labors should have produced.

Nonetheless, the main lines of Fradkin's narrative and argument are, to me, convincing and compelling. Fradkin's information comes from extensive interviewing and the testimony and voluminous documentary record generated by the lawsuits brought by downwinders in Nevada and southern Utah claiming injury from fallout.

Downwind communities were given little warning of the fallout hazard, monitoring of fallout was inadequate, reports of radiation levels were incomplete and otherwise suspect, and assessments of health effects to people and livestock were misleading if not flagrantly dishonest. Indeed, in 1982 a federal trial judge was dismayed upon revisiting testimony given in his court 25 years earlier in a case brought by sheep ranchers who claimed their flocks had been decimated by fallout. The judge was persuaded—even if the appeals court that later reversed his decision was not—that government witnesses had committed a “fraud upon the court.”

Proving that radiation from fallout has caused cancer in humans is difficult because of the prevalence of this disease and the fact that radiation-induced cancers are not distinguishable from cancers from other causes. Nonetheless, as Fradkin points out, the trial judge who heard the suit brought by downwind cancer victims ruled in 1984 that 10 of the 24 plaintiffs in the test cases before him were entitled to damages. In these 10 there had been a clear showing that the cancer suffered (principally leukemia) was one for which radiation is a known cause, that the victim was a resident of the downwind area during the period of atmospheric testing (1951 to 1962), and that exposure to fallout was not in doubt.

Had it been upheld on appeal, this ruling would have become the guiding precedent for a variety of radiation injury cases. But a central issue was whether the failure of the Atomic Energy Commission to protect downwinders from fallout hazards was exempt from damage claims by virtue of the “discretionary function” rule. The trial judge found no evidence that national policy sanctioned disregarding protection of downwinders from fallout. But an appeals court took a broader view of the discretionary function issue and

overruled him.

Fradkin views this reversal with outrage, seeing it as a perverse vindication of governmental negligence, arrogance, and deception. In fact, the record to which Fradkin refers deserves a place on the list of things contributing to public distrust of government, especially in anything related to nuclear hazards, such as disposal of radioactive waste.

The basic reason for establishing the Nevada Test Site on public desert land in the Great Basin was to provide Los Alamos with a backyard laboratory for diagnostic tests vital to weapons development. But Fradkin notes that the officials involved were well aware of radioactive fallout. Indeed, fission products from the 1948 tests in the Pacific had been detected half way around the world. But such uneasiness as there was about establishing a continental test site—neither General Dwight Eisenhower nor David E. Lilienthal (first chair of the AEC) had liked the idea—was overcome after the Soviet Union detonated its first atomic bomb in August 1949 and North Korea invaded South Korea in June 1949.

Other informative works in this field include A. Constandina Titus' *Bombs in the Backyard* (1986) and Robert A. Divine's *Blowing on the Wind* (1978). Fradkin has made a limited but useful contribution.

LUTHER J. CARTER

The Kentucky Legislature: Two Decades of Change. By Malcolm E. Jewell and Penny M. Miller. Lexington: University Press of Kentucky, 1988. 302p. \$26.00.

Kentucky's legislature, as Jewell and Miller report, has changed enormously in the last couple of decades becoming more independent, more professional, and more powerful than ever before. If other states are undergoing a similar metamorphosis (as accumulating evidence indicates they are), a new phase of legislative development may be at hand. At first the state legislature was the heart of state government and it remained so for the better part of a century. The legislature had a broad range of discretionary authority and considerable political independence. Most of the governing in domestic matters was at that time

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done by the state legislature. Legislators often dominated governors and held the initiative in policy making, at least for the limited role that government then played in society. In the decades following the Civil War, however, the reputation of the state legislature went downhill fast. Bribery and rampant corruption became common, political factions formed self-serving "state house gangs" and dealt in patronage; and in an age of much construction of utilities and other public development, legislators wallowed in "honest graft" (e.g., the buying of land for next to nothing only to sell it at an enormous profit). Some legislators, lobbyists complained, wouldn't stay bought and would sell out to the opposition just before a vote was taken. The reputation of state legislatures fell to an all-time low. The result was that in state after state constitutional restraints were placed on legislative operations. The duration of legislative sessions was sharply limited, the idea being that the less time these characters were around, the less damage they could do.

Accordingly books on state legislatures in the first half of the twentieth century tended to be devoted to muckraking or were about the formal structure and procedures of legislatures. Then in the 1950s several studies appeared that were serious examinations of the way a number of legislatures actually operated. They analyzed party and interest group activities in individual states and did comparative studies of roll call voting in a number of states.

Attention focused on legislatures in the late 1940s and the early 1950s as the so-called Little Hoover Commissions reported on the administrative systems of the states. In nearly all these reports some attention was devoted to the problems of state legislatures. Many of the recommendations were adopted by states, among them Kentucky.

In Kentucky, say Jewell and Miller, there has been a move toward greater independence of the legislative body. Whereas it was once dominated—like so many state legislatures in the South—by governors who usually chose the legislature's leadership and then used those leaders to advance their own administration's policies, gradually the length of legislative service grew, resulting in greater professionalization and legislative power. The growing stature of the legislative system helped to

minimize the unseemly practice of governors' trading local pork barrel projects for legislative votes on the governor's policies. Despite these developments, the Kentucky constitution still limits legislative sessions to 60 days every two years. It is perhaps a measure of the kind of change that has been taking place that devices have been improvised to provide as much continuity as possible, like the "interim committees" that meet regularly in the absence of the legislature from Frankfort in the off years. In general, both the regular and the interim committees have good staff assistance. With all this, the power of committee chairmen has been enhanced. The reapportionment of the legislature has brought many urban and suburban legislators on the scene making the institution more representative of the population.

In their summing up the situation, Jewell and Miller say, "The period of greatest change in American state legislatures is over. The years ahead are likely to bring more gradual change. The least professional legislatures will become more professional; there will be closer party competition in the less competitive states; and substantive issues faced by state legislatures will grow more important and more complex. This would be a good time to assess the changes of the last few decades and determine what the consequences of change have been."

I hope scholars will take up the suggestion and do the work necessary to produce useful state-by-state analyses. This book offers a high standard that I hope the authors' successors will reach for; but there is something I hope will be added in future studies: an examination of decision making on the tough questions (e.g., the regulation of strip mining and the deeds that allowed coal operators to get the right to strip mine with devastating consequences) which might help in understanding the politics of these institutions. My only reservation about the present book is that it does not show the Kentucky legislature grappling with such issues.

DUANE LOCKHARD

Princeton University

Urban Minority Administrators: Politics, Policy, and Style. Edited by Albert K. Karnig and Paula D. McClain. New York: Greenwood, 1988. 182p. \$37.95.

This book provides a solid contribution to our understanding of the role that urban minority administrators play in the formulation and implementation of policy. This book also provides helpful insights on how and why these administrators were chosen and structures our thinking about three major questions surrounding their appointments. First, should urban minority administrators be expected to be advocates of minority group interests? Second, why were these particular minority administrators hired? Third, how successful were these minority administrators in blending the traditional roles of administration with the increasing expectations of minority communities? Read together, these articles provide a useful background to answering these questions.

The introductory chapter by Paula McClain provides an excellent background and framework for analyzing the role of minority administrators and their impact on policy issues in urban settings. The question of whether these administrators can or should serve as advocates is raised here, but it is left to Henderson to discuss the issue more fully.

Lenneal Henderson's chapter provides the theoretical framework for discussing the question of advocacy. Henderson uses the concept of "role elasticity" to analyze the problems faced by these administrators. He concludes that whether one becomes an advocate depends on whether one focuses on internal or external demands of the position; that is, administrators can advance community interests by using their administrative role to confer with superiors or join and participate in community organizations outside that agency, or both. In the final analysis, the role the administrator chooses will be tempered by the agency and by personal ambitions. While Henderson's work is useful, it is not exhaustive.

In responding to the second and third questions, there are some common themes that run through the articles. First, political changes preceded the selection of these administrators. The Civil Rights movement set in motion a wide range of challenges to urban authority

structures, including the election of minority officeholders. Their election, plus increased pressure from minority communities, led to the appointment of more minority administrators. This is more clearly stated than in the article by Reuben M. Greenberg (chief of police of Charleston, South Carolina).

The second theme is that these administrators were competent and had a wide range of experiences before taking the top job. Thus, selection was not just based on competence, it was also based on the unique experience these minority applicants brought to the job. Another important point needs to be made here, namely, in each case some white administrators or elected officials became "risk takers" and helped make these selections. It matters little whether these were self-serving or for altruistic reasons. The fact is that this type of selection is frequently avoided by most white elected officials and administrators.

The third theme deals with community expectations. Invariably, some members of the minority community have unrealistic expectations of these administrators. The essays in this volume all speak of developing *trust* with these groups and providing *access* to them to improve lines of communication. The articles by Davis and Valdez both deal extensively with this issue.

The fourth theme concerns decision-making styles and strategies. Most of these administrators believe in some form of participative management. It is also clear that while each of these administrators actively pursues affirmative action hiring, retention, and promotion, they don't believe in wearing on their sleeves the fact that they are minorities; that is, incompetent people, whether they be black, white, or Hispanic, are not pushed by any of these administrators.

The consistent message in this volume is that while urban minority administrators share some unique characteristics, they must find ways to succeed despite additional burdens placed on them by the bureaucracy, minority communities, and the mass media. The essays by Murray and Greenberg are excellent and provide considerable detail on some of the general problems faced by minority administrators in a wide variety of situations.

Karnig and McClain conclude with an excellent summary of some of the similarities found in the essays. However, the point needs

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to be made that not all of their conclusions are specific to minority urban administrators—for example, the ephemeral nature of a public administration position is universal, since both minorities and nonminorities are appointed to these positions.

Each of these essays is worthwhile reading. The articles do not answer all of the questions raised by the topic, but they do provide some valuable insights based on experiences of successful people. Since this is one of the few books on this topic published in the 1980s, it will be important reading for students, administrators, academics, and people in leadership roles in black, Hispanic, and Native American communities.

MICHAEL PRESTON

University of Southern California

Baby Boomers. By Paul C. Light. New York: W. W. Norton, 1988. 317p. \$19.95.

Paul Light sets out to assemble a portrait of the baby boom generation. He attempts to demonstrate the generation's uniqueness and the sociopolitical problems that are likely to confront this generation in the future. He defines the generation as those born between 1945 and 1964. Light points out that the sheer size of the generation, 75 million, means that there is tremendous diversity within it. It is far from monolithic. The generation is characterized by racial, gender, class, and educational divisions. Moreover, those who were born at the beginning of the boom have undergone a significantly different set of life experiences from those born at the end of the boom. Journalists have tended to focus on yuppies when writing about the baby boomers, implying that the generation was marked by extreme affluence. Light demonstrates that yuppies constitute a minority of the baby boomers, only 5% fit the definition.

Although the generation is marked by diversity, Light argues that baby boomers do share a common set of attitudes or life orientations as a result of their experiences and that this distinctive subculture has implications for its political attitudes and behavior. Light describes a generation distinct not only as a

result of political events like Vietnam, Watergate, the Kennedy assassinations, and the Civil Rights movements but one that has been influenced by the presence of television, the threat of nuclear war, and urban congestion. Light suggests that these experiences have produced a generation superficial in its treatment of candidates and deeply concerned about themselves and their privacy. As a result of television the generation is satisfied with shallow campaigning and a politics that emphasizes symbolism over substance. Light argues that because of the possibility of nuclear war, the generation focuses on the present and seems incapable of planning for the future; while congestion has yielded an intense focus on privacy. Moreover, Light maintains that this generation has separated itself from the past, is less attached to social institutions and roles, and is reluctant to make commitments. The generation is highly individualistic in its orientations.

Light relies almost exclusively on the research of others. He attempts to integrate the works of numerous social scientists to construct a picture of the generation. Unfortunately, for those interested in public opinion and voting behavior in general and generational effects in particular, Light's book is unsatisfying. The book does little to advance our knowledge about generational effects. His focus on how changes in life-style affect political style is interesting, but he offers an argument without empirical evidence to support it. Indeed, many of Light's statements about baby boomers are observations about how they *may* differ from previous generations. He offers very little empirical evidence that most social scientists would be willing to accept with any degree of confidence. For example, age differences for a policy issue or political orientation are presented without any attempt to determine whether such differences represent generational or life cycle effects. Overall, methodologists will be disappointed in this book. As such, the book is often highly speculative in its descriptions of the uniqueness of the baby boom generation and its future. For the areas in which empirical evidence on the political orientations of the young are presented—lack of partisanship, trust in government—Light presents findings that are familiar to most scholars in the field.

Although I found his discussion about the

importance of life-style changes for political attitudes overly speculative, I nevertheless found parts of the discussion intriguing. When we study generational effects, we typically focus on events occurring in the political environment, ignoring social changes that are not overtly political in nature. Additionally, Light has an insightful discussion of the problems with the social security system. Because the baby boom generation is large in size and life expectancy is increasing and since baby boomers are having fewer children than their parents, severe strains on the social security system appear unavoidable.

JEFFREY W. KOCH

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Bicameral Politics: Conference Committees in Congress. By Lawrence D. Longley and Walter J. Oleszek. New Haven: Yale University Press, 1989. 361p. \$50.00 cloth, \$16.95 paper.

Everything you ever wanted to know about congressional conference committee processes is right here in this book. Lawrence Longley and Walter Oleszek have teamed up to write an exhaustive synthesis of research and commentary about conference committees. It is, as Richard Fenno writes in the introduction, "a kaleidoscopic look."

Above all, these authors exhort students of Congress to pay attention to conference committees. Fewer than one-fourth of all bills go to conference, and perhaps one-eighth of all public laws emerge from the conference route. But these are major bills leading to important laws; thus, the authors argue, the conference committees are "central arenas of legislative power." Accordingly, the authors rehearse the justification for a Congress of two houses to establish the institutional envelope for their inquiry. Because conference committees are, by definition, composed of members of both House and Senate, it is impossible for the authors to avoid a "bicameral perspective."

Longley and Oleszek eschew the traditional research question posed by students of conference committees—Who wins?—and pursue the more nondirectional question, What goes on? The heart of the book provides a portrayal of

the main actors in the conference process, describes preconference processes, elaborates and illustrates conference committee bargaining, and details the aftermath of conference actions. The authors draw these stages of the conference process together at the end by offering nine brief but interesting case studies.

Exhaustive as it is, this study has its limitations. Some are minor. That conference politics is important and that bicameralism is the essence of Congress are quite valid assertions, but these exhortations are repeated rather tediously. In the same stylistic vein, I found the persistent practice of using quotations—some quite mundane—as chapter and section headers too contrived. But it is to the authors' credit that they did not repeat Woodrow Wilson's excessively quoted aphorism about congressional committees ("Congress in its committee rooms is Congress at work") until page 109.

While the authors several times acknowledge the much greater incidence of multiply referred bills in the 1980s and the resulting much larger conference committees, their analysis is largely limited to conferences made up of House and Senate members of the parallel pair of committees. Longley and Oleszek note with exclamation points that the 1986 conference on budget reconciliation included 242 conferees from the two houses. In the first summer of the 101st Congress the conference committee on the savings and loan bailout bill included 102 conferees, a number so large that many thought the capacity to resolve the issue would be hampered. While recognizing that differences in the sizes of conference committees might affect deliberation, the authors present little analysis of the impact of size on conference outcomes.

Equally limiting is a lack of sustained consideration of the role of staffs in conference negotiations and decisions. It is known that sometimes House and Senate conferees do not actually meet but that agreement is hammered out by the key staff players. Longley and Oleszek take note of, and illustrate, staff influence in conference; but the claims are established by assertion rather than by empirical analysis. Their observations about staff influences cry for future systematic studies of the role that staff actors do, in fact, play in the conference process.

Longley and Oleszek's heavy reliance on the

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extant literature about Congress sometimes leads them astray. This appears strikingly in their assertions about the weakening of party leadership in Congress, especially the House. In contrast to the authors' claims, I believe careful research would show that House leaders carry much more influence in conference outcomes in the 1980s than did Speaker Rayburn in the 1950s. By the same token, I think the authors' claims about the consequences of subcommittee fissiparousness are off the mark. At a minimum, their argument points to the need for much more substantial research on the influences of party and committee leaders on conference decision making.

Today, congressional conference committees are open for systematic analysis. Longley and Oleszek have performed a valuable service; they have given us the intellectual and descriptive basis for future research on conference committee behavior. It is up to other scholars to take up these cudgels.

SAMUEL C. PATTERSON

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Public Opinion and the Supreme Court. By

Thomas R. Marshall. Boston: Unwin Hyman, 1989. 214p. \$39.95 cloth, \$14.95 paper.

Should or does the Supreme Court represent the views of the majority of the public? This question, as Marshall notes, has been a persistent bone of contention within the Court and more generally among political thinkers since the beginnings of the republic. Some, such as Hamilton, have cast the Court as an important bulwark against intemperate and transient public opinion. Others have argued for a Supreme Court deferential to majoritarian views, "restrained" in its willingness to challenge legislatures. And yet "almost all constitutional scholars and democratic theorists agree that the Supreme Court is, either in process or in substance, a countermajoritarian institution" (p. 4).

That scholarly consensus marks Marshall's point of departure. Formally, of course, the Court is countermajoritarian. But as Marshall argues, the Court, although countermajori-

tarian in form, may nevertheless broadly reflect the opinions of the majority, especially if Congress or the state legislatures do not represent the popular will. Thus, Marshall suggests an empirical examination of the fit between the Court's decisions and the sentiments of the public. His strategy of inquiry consists of comparing the substance of the Supreme Court's decisions and the results of polls of the public on these issues taken more or less contemporaneously. From the advent of polling during the 1930s through the present, Marshall manages to pull together 146 cases in which he can match a decision of the Court with the results of a poll taken on a national sample. This book is an extraordinarily thorough analysis of the empirical linkages between the Supreme Court and public opinion. Marshall attacks the issue from every angle.

Marshall in chapter 4 attempts to estimate and account for the degree to which the Supreme Court's decisions represent the views of U.S. public opinion. He found consistency between the actions of the Supreme Court and public opinion in a clear majority of the 146 cases from 1935 through 1986. Accordingly, he concludes, "The modern Court appears neither markedly more nor less consistent with the polls than are other policy makers" (p. 80). Marshall tests a number of competing explanations of the Court's agreement with public opinion, and some of them do quite well. Thus, for example, during times of crisis, the Supreme Court reflected public opinion more accurately than at other junctures.

The problem of conceptualization strikes me as serious. He does not specify a persuasive model of representational linkage. Why should we anticipate a relationship between public opinion and the Court's decisions? Should public opinion have a direct impact on the Court or will other forces mediate? The high level of aggregation poses severe problems for the study of the Court and representation of public opinion. For the Court on most issues, as for other political actors, particular constituencies (e.g., blacks) will probably loom more importantly than undifferentiated opinion at the national level. Unless we can move downward to lower levels of aggregation, I think we will continue to have trouble in trying to connect the Court and public opinion.

Later, Marshall asks whether "Supreme Court decisions favorably influence mass

public opinion" and anchors his research firmly in the tradition of Black (1961), Bickel (1957), and others who have argued for a legitimizing role for the Court. Marshall compares 18 decisions of the Supreme Court from the mid-1930s through the present with the aggregate results of polls taken before and after the decisions. Polls shifted as a result of the 18 decisions; but overall, Marshall reports little evidence of a consistent direction to change. Over 15 years ago, Adamany made a strong empirical case against the existence of a legitimizing role for the Supreme Court. Marshall's results seem to confirm Adamany's (and others') argument.

Here, again, we encounter problems of data. Even in those instances in which pollsters have left a sufficient trail of observations, the wording of the items and the timing of the survey often leave much to be desired. For example, sometimes the phrasing of an item did not adequately reflect the nuances of the Court's decision; and the space between the date of the decision and the taking of the polls varies wildly. Naturally, pollsters choose issues high in salience. Accordingly, it is unlikely that the decisions for which we have data from polls constitute anything like a representative sample of the Court's jurisprudence. Some of the juxtapositions seem curious. For example, the similarity in the short-term effects of the decisions on capital punishment (an issue of high salience) and on payment of foreign debts (a matter of low visibility) strikes me as peculiar. Many readers will, like me, worry about the small number of cases Marshall uses in these analyses, with the attendant possibility of unstable estimates.

Does the Supreme Court represent public opinion? Can and does the Supreme Court shape public opinion? The answer to each question, I think, is that we do not yet have sufficient evidence in which we can place much confidence. This should not reflect poorly on the scholars working in this vineyard. The problems of data here are severe, perhaps insuperable; and the enterprise remains young enough to justify exploratory efforts. Marshall exhibits an admirable boldness in collecting, analyzing, and interpreting these data from diverse polls and a variety of issues. It is not an enterprise for the faint of heart. Some who read this book will take issue with some of the choices Marshall has made. Yet for the near

future, Marshall's book constitutes the best evidence we have on the relationship between public opinion and the Court.

GREGORY A. CALDEIRA

Ohio State University

The Generous Corporation: A Political Analysis of Economic Power. By Neil J. Mitchell. New Haven: Yale University Press, 1989. 163p. \$18.50.

One of the unusual features of the United States is that its citizens are privately quite generous. Tocqueville noticed this, and so have many modern students. Exactly why U.S. citizens are more likely than others to make gifts to universities and so on is a mystery.

In the nineteenth century, these individual gifts were the rule; but with the development of the corporate form, it was to be expected that the same policy would be taken up by corporations. Mitchell's book chronicles the original development of this attitude in those corporations. Although he deals a little with the nineteenth century and the period after 1930, basically the book is concerned with the first three decades of the twentieth century.

The normal explanations for the corporations'—or for that matter wealthy people's—making gifts are partially the mild altruism that almost all of us have and partially the desire for good public relations. In the case of corporations that provide special facilities for their workers, it is normal to add on to these two arguments the desire to improve the quality of their labor force by reducing turnover, improving morale, and so on.

Mitchell accepts this more or less, although he uses the more technical term *achieving legitimacy* for good public relations. He also, however, emphasizes that the companies that do this tend to be large companies that have recently had trouble with the government and are highly conspicuous. Further, he thinks that most such companies generally have at least some monopoly power and thus something to defend. He cites a slogan of the Medici: "Money to get power, power to protect money" (p. 53).

It can be seen that this book is not entirely based on the premise that companies are at-

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tempting to do good. He does, however, point out that the efforts to be nice, to be altruistic, and so on are surely an important part. In particular, he uses statistics to argue very strongly that the companies did not adopt this kind of program either to prevent unionization or to prevent development of government programs. He points out that these developments were at their high point in the twenties when neither of these menaces was very important.

Overall, it is a good history of the period and I think achieves the objective of the author, which is to provide political science tools for the benefit of economists. I do wish he had spent rather less time on the now-moribund Berle and Means theory of corporations. Mainly, he uses considerable data to demonstrate that the management-controlled corporation is hard to define and, in any event, does not have much affect on corporate generosity. I cannot say that he is wrong; nevertheless I would have preferred he had used the more modern market-for-control theme in dealing with corporations.

Altogether, this is an interesting, well-written, and short book. I read it in an afternoon and I think many economists and political scientists will be well advised to devote an afternoon to the same pursuit.

GORDON TULLOCK

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The Demise of Nuclear Energy? Lessons for Democratic Control of Technology. By Joseph G. Morone and Edward J. Woodhouse. New Haven: Yale University Press, 1989. \$22.50 cloth, \$6.95 paper.

Collapse of an Industry: Nuclear Power and the Contradictions of U.S. Policy. By John L. Campbell. Ithaca: Cornell University Press, 1988. \$32.50 cloth, \$11.75 paper.

Democracy in the Shadows: Citizen Mobilization in the Wake of the Accident at Three Mile Island. By Edward J. Walsh. *Contributions in Sociology*, no. 72. New York: Greenwood, 1988. \$39.95 cloth.

Morone and Woodhouse set out to examine "the ability of representative government to manage complex technologies sensibly" (p. 2). Nevertheless, their book is not so much an

analysis of politics and technology as it is a proposal of what should be done to restore the nuclear option. The authors believe that nuclear power could be acceptable if only the correct type of reactor were selected. In making this point, they present numerous arguments whose flaws appear conspicuous in light of the analyses offered by Campbell and Walsh.

By attributing the demise of nuclear energy in the United States to the wrong choice of reactors, Morone and Woodhouse overlook the political-economic forces that shaped the development of nuclear power in the United States and offer a "technical fix" that trivializes the problems created by the use of nuclear power. By slapping politicians and regulators for selecting the wrong reactor design—one that favored rapid commercialization rather than safety—they miss the central intent of the U.S. nuclear program. Campbell, on the other hand, shows how national hubris, military interests, federal regulators, incentives, and subsidies drove rapid commercialization. Within the context of the private corporation and competitive markets for capital, he shows how the goal of commercialization favored an off-the-shelf technology and the rapid scaleup of military reactors, which eventually made it impossible to assure public safety and garner public acceptability. Morone and Woodhouse never stop to consider how safety could have been the central concern of a program initiated for military reasons, promoted by the federal government as critical to national security, and developed by private corporations in an economy driven by profit and with a short-term investment horizon.

Second, Morone and Woodhouse easily fall into the traps of the pluralists and technocrats. They confuse lack of participation with consensus (p. 120) and lament the failure of the U.S. political system "to modify a multibillion-dollar program that was becoming politically unacceptable" (p. 21). Yet nuclear power was spawned and sustained by this political system, which its critics were relatively powerless to modify.

This point is strongly made in Walsh's account of Three Mile Island, where, despite the protests of local communities, congressional delegations, and the governor and unresolved questions about the integrity of the utility's management, the Nuclear Regulatory Com-

mission (NRC) approved the restart. Walsh details the difficulties, costs, and commitment required to mobilize citizen opposition to a pronuclear government-industry front even following the nation's worst reactor accident.

Nevertheless, Morone and Woodhouse see "political passivity" as allowing "technological inertia to force unsatisfactory policy options on society" (p. 155). They refer to "technological opportunity driving society" (p. 119) and "technological momentum" as "inevitable" (p. 128). They never consider that technical inertia is created by public and private institutions with the power to protect their investment decisions by overriding criticism and opposition. If technology drives society, it may be because technology is an indispensable part of competition in capitalist society, which is often at odds with the interests of public health and safety—a proposition at the center of Campbell's book.

Morone and Woodhouse conclude that federal ownership could improve the prospects for nuclear power by forcing standardization of reactor designs. But federal control of nuclear power could also make it even easier to steamroller public opposition, as Campbell points out in his discussion of the success of nuclear power in France. Federal ownership may make development less subject to the competitive forces that result in less-safe reactor designs. At the same time, one must recognize that if nuclear power had been unmercifully exposed to competitive forces from the very beginning—no Price-Anderson Act, no federal demonstration projects, no offers of waste disposal, no federal incentives—we would probably not be concerned with the problems created by commercialization today.

Third, Morone and Woodhouse assume a distinction between an industry and its regulators when they propose that regulators could have insisted on more experimentation and slower commercialization. The promoters and regulators of nuclear power, however, were one and the same until the Atomic Energy Commission was disbanded. And as Walsh documents, even after those competing mandates were supposedly disentangled, most NRC commissioners were hardly reticent in their advocacy of nuclear power. Morone and Woodhouse themselves advocate "inherently safe reactor" designs as the answer to the climatic and health impacts of the fossil fuel

cycle (mining, etc.). But this often-heard proposal of the nuclear industry ignores the impacts of the nuclear fuel cycle. Nuclear power also looks less appealing when one considers the health effects of uranium mining, the need for huge amounts of electricity (often from coal-burning power plants) to produce enriched uranium and other components of nuclear reactors, and the uncertainties of radioactive waste disposal.

While discussed here as correctives to arguments made by Morone and Woodhouse, the Campbell and Walsh books are excellent analyses in their own right. Of hundreds of books and articles discussing the failure of nuclear power Campbell's is the best. Not only does he discuss the myriad of explanations already found in the literature, he presents a more thorough and insightful analysis of the political, economic, and social developments that factored into the demise of nuclear power. Discussion of the political economy of nuclear power in European countries is used to further strengthen his observations about the U.S. case. As an author, Campbell anticipates many of his reader's questions and provides comprehensive, up-to-date documentation.

Walsh offers important insights into the institutions and politics that have kept nuclear power going in this country long after its failures became obvious. His examination of the Three Mile Island accident shows the roots of the credibility and legitimacy crises that continue to plague the nuclear industry, its regulators, and its promoters in the federal government (especially the Department of Energy). In doing so, he highlights fundamental problems with the continued use of nuclear power in the United States and with attempts to manage its legacy of radioactive waste.

GERALD JACOB

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Videostyle in Senate Campaigns. By Dorothy Davidson Nesbit. Knoxville: University of Tennessee Press, 1988. 193p. \$24.95.

Most scholarship on political campaigning over the last few decades has been quantitative in approach, particularly with regard to the

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content and effects of media. Running parallel with such research and sometimes converging with it has been a considerable amount of high-quality campaign journalism (e.g., Theodore White, Elizabeth Drew, Martin Schramm). The book under review is neither quantitative in approach nor "journalistic," but is self-consciously a work of qualitative social science. Nesbit, a political scientist who works in the advertising industry, has undertaken a study of how television advertising was conceived and executed in three U.S. Senate races in 1982. This she does by means of lengthy interviews with principal actors (candidates, managers, producers) and impressionistic scrutiny of packages of paid spots provided to her by the campaigns. The author makes clear at the outset that she is interested neither in systematic content analysis nor in the precise assessment of media effects on voters. One of her premises is that political scientists ought to know more about how and why campaigns decide to present candidates as they do in television spots.

Nesbit opts to connect her work with Fenno's concept of homestyle, although she takes pains to argue that videostyle is in fact quite different because it is based on collaborative effort and does not produce immediate feedback for the candidate. While it is understandable that she might find comfort in drawing on Fenno, the distinctions drawn between homestyle and videostyle are close to self-evident. A question that might have been explored more explicitly is how long-term homestyle constrains campaign videostyle, and vice versa.

Rejecting the familiar notion of a horse race, Nesbit offers us the metaphor of courtship to describe the phases of campaigns, with candidates as suitors and voters as the objects of attention. Without denying the potential value of metaphor, I would suggest that courtship works here only in the most superficial sense. Nesbit herself acknowledges early on (p. 20) that the metaphor is structurally flawed. We do not need the concept of courtship to appreciate Nesbit's important points that candidates need to build "intimate" relationships with voters and impart relevant information to them. Indeed, for social scientists metaphors are most valuable when they actually serve to describe the thought processes of the actors. In the cases at hand it is clear only that Nesbit

likes the metaphor, not that decision makers employ it.

The case studies themselves are intriguing but frothy. Nesbit strives to protect identities. She dubs the candidates with names such as Senator Matt Niceguy and Johnny Fasttrack, while states are called Magic State, Sunstate, and Outland. All of this is ethically commendable but does not serve to mask identities all that well. In light of the effort to preserve anonymity, I was surprised that the interview excerpts reported in the book were not more candid. Much of what the respondents told Nesbit looks indistinguishable from what one might expect "on the record."

The author claims that she is trading breadth for depth in the research, yet the need to mask identities constrains her from developing the range of contextual information that would allow the reader to judge better the soundness of her (cautious) generalizations. She tells us mostly about how television advertising takes shape, only a little about broader campaign decision making and organization. We learn scarcely anything about voter response to the ads, except through secondhand impressions. In the end, the slice of political reality that is exposed is just a bit too narrow, and the reader is left thinking that there is a good deal more going on here than this approach yields.

If one is prepared to settle for some preliminary testable hypotheses about what does and does not work in Senate media campaigns, the book is useful. Unfortunately, the author chooses to articulate these hypotheses in her final chapter in the form of 10 "commandments" of running for the U.S. Senate, for example, "Thou shalt give people a reason to vote for you that is consistent with your strengths as a candidate" (p. 157) and "Thou shalt never tell, always show" (p. 164). The commandments derive—more or less—from the case studies; but campaign professionals and scholars could argue at length about what is entailed by each.

Nesbit's objectives are sound, and her qualitative orientation is perfectly agreeable. The book is less than a complete success because the case studies are not deep enough and the context inadequately established. Failing to achieve the requisite depth, it would perhaps have been better had she been able to expand the number of cases, thus strengthening the basis of comparative generalization. For all my

reservations, I learned something valuable about media campaign strategizing from the book; and I am inclined to believe that the concept of videostyle will have a place in future research, assuming Nesbit and others can fine-tune it a bit.

CHARLES M. TIDMARCH

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Environmentalism and the Future of Progressive Politics. By Robert C. Paehlke. New Haven: Yale University Press, 1989. 325p. \$25.00.

The search for the political meaning and direction of contemporary environmentalism has become a topic for politicians; newspaper columnists; the large, staff-directed, Washington-based environmental organizations; corporate lobbying associations; and now, most recently, academic analysts. Environmental issues, including the more mundane and locally oriented problems such as garbage and sewage and the international-scale, paradigmatic concerns such as the ozone layer and global climate change have risen to the top of policy agendas. Opinion polls indicate even more substantial majorities expressing support for environmental programs (including a willingness to pay), which have long been the barometer for justifying expensive, after-the-fact or postproduction policies. Environmentalism in this context becomes the potential basis for either a new politics or—as the logic and strength of the existing political discourse seems to demand—a reformulation of the old.

This concern for reshaping politics lies at the heart of Robert Paehlke's *Environmentalism and the Future of Progressive Politics*, an interesting but ultimately disappointing attempt to define the parameters of what Paehlke sees as the basis for the first new ideological perspective since the mid-nineteenth century. The author, a professor of political science and environmental and resource studies at Trent University in Peterborough, Ontario analyzes the experiences of both U.S. and Canadian environmental groups and several of the key issues they have addressed, including population growth, energy use, and resource scarcity, as well as the values and ideological assump-

tions that have framed the environmental response to urban and industrial development. He sees the emergence of the environmental movement of the 1960s and 1970s as essentially an apolitical or even antipolitical reaction to the new issues of pollution above and beyond the traditional conservationist agendas.

Paehlke argues that environmentalism still needs to confront a powerful, neoconservative backlash, much of which he sees as still central to the political debates of the 1980s and 1990s. In this context, Paehlke's more expansive and *political* environmentalism—capable of addressing such diverse concerns as reducing work time; paring down the military budget; increasing expenditures for education, social welfare, the arts, and health; modifying the prerogatives of the market; and expanding the basis for environmental intervention—can provide the appropriate alternative in the developing postindustrial era. This form of politics, which Paehlke identifies as moderate, progressive, and left-of-center (or rather as a "center that holds"), is seen as capable of constructing the winning coalitions and electoral strategies necessary to bring about such changes.

In some essential ways, Paehlke's prescriptive outline of this new politics is similar to the programs that have been discussed—and, in several documents, put forth—by a number of the conventional environmental organizations he sees as nonpolitical. These groups, many of whom have coalesced around such agenda-generating documents as Cahn's edited *An Environmental Agenda for the Future* (1985), Maize's edited *Blueprint for the Environment* (1988), and the Natural Resources Defense Council's *America's Economic Future* (1984), see themselves as political players—not just environmental issue coalition builders but general practitioners in the arts of lobbying, electoral maneuvering, and issue trade-off and compromising that characterize "politics" in 1989 North America.

It is precisely this notion of politics, however, that has come to be challenged by a broad range of grass roots movements and organizations who have experienced the environmental consequences of industrial and urban activities in their "own backyards." These movements, while not yet constituting on this continent an autonomous political force, such as the Greens in West Germany, have never-

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theless changed the shape of North American environmental politics in the late 1980s. These social movements are not simply a reflection of new "ideas," they have become part of the process of shaping such ideas. Indeed, what *Environmentalism and the Future of Progressive Politics* suffers from most is its narrowly drawn connection between ideas and action: How does the contemporary history of these differing ideological expressions reflect on, and how is it reflected by, the various social movements that have emerged to articulate a new environmental vision? In this regard, contemporary environmental politics still awaits the kind of incisive analysis that Samuel Hays presented with respect to the turn-of-the-century movements in his masterpiece, *Conservation and the Gospel of Efficiency* (1959).

ROBERT GOTTLIEB

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The Ring of Power: The White House Staff and Its Expanding Role in Government. By Bradley H. Patterson, Jr. New York: Basic Books, 1988. 382p. \$19.95.

Affiliated now with Brookings, Bradley H. Patterson, Jr. has been an astute participant-observer in the White House under three Republican presidents through a span of 14 years. His experiences and the voluminous files he has evidently accumulated enabled him to write an anatomy of the president's staff and most of its offices and to annotate the functions of the almost four thousand people, military and civilian, who are appointed, detailed, or assigned there. Through some two hundred interviews with former White House personnel, Patterson extended his information base. He then used these materials and his own experiences to enrich his writing in a blended narrative that appears to be intended for general reading as well as for the student of White House staff organization.

Two objectives are stated: first, "The purpose of this book is . . . to demonstrate why the modern White House staff exists, and to afford a close-up look at what each element of the contemporary staff is and does" (p. 6); and much later, "It is the thesis of this book that the development of the modern White House

staff has become a necessary part of [the president's] strengthening" (p. 349).

Moving straight forward through 340 pages from the first point to the last, Patterson implies that each of today's staff components is nearly indispensable for a strong presidency.

The text has four parts. Part 1, "The Cabinet Government That Isn't," carries these arguments: most national issues are interdepartmental and interagency; both foreign and domestic crises require command center management at 1600 Pennsylvania Avenue; and electronic communication developments have increased, and will continue to increase, the frequency and the speed of direct communication between the expanded and updated Situation Room and the government, the rest of the country, and the world. This is a remarkable point of view for Patterson, who served and supported Eisenhower's use of limited cabinet government in the fifties and who wrote *The President's Cabinet* for the American Society for Public Administration in 1976. That was a time when many thought cabinet-type government was an antidote for White House staff sins.

Apparently satisfied that cabinet government with a necessarily weaker president has been discredited, Patterson moves on to part 2, "The Bashful Bureaucracy." Here, most of the book is devoted to describing 16 established offices in the West Wing and East Wing and in the Old Executive Office Building. Notably (and questionably), the Office of Management and Budget and the Council of Economic Advisers are not included. Each chapter is devoted to an office and describes "who *does* what, when, how." Chapters range in size from 37 pages for the National Security Council to a snapshot seven pages for the Scheduling Office. Here is the contribution to our literature on the White House staff. Using anecdote and quote Patterson emphasizes the assignment of specific responsibilities and the accompanying growth of presidential demands for assistance—and the need, as the author sees it, for such growth.

Part 3, "The Professional White House," reminds that inauguration does not completely empty the structure. There are memory keepers who preserve the past and provide for the continuity of communication services. The executive clerk, the telephone operators, the correspondence office, the military office, the

secret service, and the residence staff are touched on. Again, Patterson's thrust is toward the institutional and equally toward growth and cohesion.

In a seven-page conclusion there is rhetorical support for the presidency as the center of national government, the staff as the strength of the president as he governs, and the White House as the only centripetal element in an administration. Departments and agencies, according to the author, tend to pull things apart as they pursue their own needs and plans. The departments preserve the established routines and responses generated in the past. It is the president, through his staff, who must generate executive power. A loyal and ethical staff should convert the president's guidance and policy, in routine as well as crisis situations, into the driving force expected of the executive branch.

This is not the usual memoir of days gone by. There is purpose in Patterson's prose, and there is purposive description in his catalog of staff offices and activities. Hence, his approach and the resulting publication supplement the writings of such political scientists as John Kessel, Larry Berman, Stephen Hess, Paul Light and Roger Porter.

Patterson's interviews, full and credible as they are, raise one question of style, however. There are well over a dozen quotations, several lines in length, from interviews that are footnoted as "off-the-record interviews with the author." Some of these citations give neither place nor time of the discussion. The time is often significant when past, recent, or current staff members are asked to remember. In many instances the quotations are significant. Once published, these are on-the-record according to the rules of journalism. They may be anonymous or not-for-attribution, of course.

Despite this caveat, Patterson's work is of scholarly merit. It is a different approach, and it fills current gaps.

GEORGE GRASSMUCK

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The Competitive City: The Political Economy of Suburbia. By Mark Schneider. Pittsburgh: University of Pittsburgh Press, 1989. 249p. \$34.95.

Mark Schneider has established himself as a leading empirical analyst of suburban politics in the United States. In this text he adds considerably to his already-substantial contribution to the large-scale, comparative, and quantitative study of suburban municipalities. *The Competitive City* integrates in one forum a number of important conceptual, theoretical, and empirical questions. Schneider's determination to examine rigorously hypotheses regarding the substance, mix, and cost of local public services provides a number of important findings, not all of which are expected.

Although the author has a distinct point of view, discussed below, he deserves praise for this fruitful effort to connect conceptual-theoretical questions with empirical analysis. Perhaps in the analysis of suburban politics more than elsewhere, there has developed a tendency to insulate theory from empirical analysis. For too many of us, it seems adequate, on the one hand, to make ideologically driven pronouncements regarding either the vices or virtues of contemporary suburban life. On the other hand, there is the antiseptically generated quantitative analysis of suburban municipalities, producing a collection of anecdotes about the relationship of one kind of lower-order measure with another (e.g., municipalities with higher election turnouts tend to be more affluent). By employing theory while simultaneously confronting the messiness of reality and the challenge of hypothesis testing, Schneider joins such scholars as Bryan Jones and Martin Shefter, whose works are characterized by a desire to confront empirical evidence with interesting theoretical, even normative, topics (e.g., equity in the distribution of public services or alternative theories regarding the onset of fiscal stress).

In *The Competitive City* Schneider applies a number of concepts drawn from Tiebout's analysis of the territorial organization of metropolitan United States. Tiebout argued that metropolitan jurisdictions would provide alternative packages of services, in response to the demands of mobile households seeking to maximize self-interest by locating in places that

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enhance their satisfaction. Consequently, according to this view, metropolitan jurisdictions are comprised of different packages of services and burdens, depending on the varying tastes and differing levels of resources that competing households, business, and jurisdictions have to invest in seeking, retaining, or enhancing various metropolitan locations.

In perhaps the most important aspect of this work, Schneider builds on Peterson's *City Limits* (1981) by claiming that suburban localities are not completely free to respond to the demands of households or firms. Neither are they free to deploy governmental resources in whatever manner desired. Schneider also incorporates Peterson's distinction among allocational, developmental, and redistributive expenditures. Schneider goes on to suggest that among the important local actors placing demands on the suburban regime are its public employees. Building on the budget-maximizing perspective of public employees associated with such names in public choice theory as Stigler and Niskanen.

Although it is impossible fully to array the hypotheses that Schneider generates from his eclectic use of these several theoretical approaches, his overall conclusions are relatively easy to summarize. Specifically, after a carefully executed regression analysis of a number of alternative relationships and reviewing the many ways in which relationships might be conditioned, Schneider concludes, as Peterson and many others before him have claimed, that suburban expenditures are constrained by law and by the social makeup of households. But Schneider also points out that regardless of expenditure category, communities are constrained by interjurisdictional competition in their expenditures. This quick summary does injustice, however, to the adroitness and richness in detail that Schneider's work provides.

It is worth relating a number of important problems that are not addressed in this book. Schneider adopts without much justification the criterion of lower costs in public services as the basis for ascribing efficiency. However, there are situations when a locality spending less is behaving less efficiently, as when it underinvests in the future regarding infrastructure or the training of its children. Moreover, higher expenditures are in some instances capitalized into higher values for

private property, as in the case of school expenditures, whose costs are often exceeded by the gain in higher property values. It would have been helpful, then, if the author had included a more general discussion of the conflicts among different ways of evaluating the performance of metropolitan suburbs. These are very minor criticisms, however, of a book that deserves widespread attention among urban scholars.

MAX NEIMAN

University of California, Riverside

Recruiting for Uncle Sam. By David H. Segal. Lawrence: University Press of Kansas, 1989. 221p. \$25.00.

Arms and the Enlisted Woman. By Judith Hicks Stiehm. Philadelphia: Temple University Press. 331p. \$29.95.

Segal and Stiehm have written timely texts. Segal's is the more comprehensive, historically rich account, and he tracks the question of citizenship obligation and military service in a direct and perspicuous way throughout his discussion. Debates over citizenship rights and responsibilities fade into the background of Stiehm's discussion. She is primarily interested in making "America's most unknown soldiers," enlisted women, visible. Although Stiehm begins in a promising way by asking what prompts some people to "sacrifice their lives for others," the sacrifice theme is never confronted head-on, perhaps because Stiehm nowhere comes to grips with the perplexities of national identity and loyalty.

Stiehm asks "whether men's monopoly on the role of warrior undermines women's claim to be full citizens" (p. 3), but the prism through which she views citizenship is driven more by rights and entitlements than obligations and responsibility. She begins by assuming that "men in our culture are trained to expect private and governmental, legal and illegal violence" (p. 2)—a debatable proposition, although she treats it as a *prima facie* assumption. Is it really the case that all U.S. males, from the inner city street tough to the middle-class college student, *expect* private violence? And what happens to our understanding of

very diverse phenomena if we put drug-related gang warfare on a linear continuum with those complex engagements between nation states we call "war"?

Stiehm would like to see "manhood" severed from "military." If the military is no "longer a way to demonstrate manhood" (p. 7), those lingering stereotypes that militate against women in the military will dissipate; for enlisted women at the moment endure, on Stiehm's account and in light of the evidence she has amassed, harassment, "a good deal of stereotyping," and male commanders who experience "discomfort with women." Because the women know that some men are "wimps, goof-offs, or hoodlums," they chafe against assumptions that restrict their full military participation. "Women believe that they do a good job and are discriminated against" (p. 105), even as military men believe standards have been altered and dramatic exceptions made in order to absorb women into many military roles.

On the matter of women in combat, Stiehm seems to waver, perhaps because she sees military service in individualistic terms. Thus, she argues that some military women want to engage in combat and others do not: "They are opposed both to being barred from and being forced into combat; women do not want to be coerced" (p. 105). This seems a rather odd attitude from individuals who have presumably absorbed an intense collective ethos. Military service has always been about discipline and, yes, coercion. War fighting is a group, not an individual, affair and relies heavily on high morale in small, cohesive units.

Perhaps the attitude of the women Stiehm surveyed is not so odd after all if one brings Segal's forthright discussion to bear. Segal puts us inside this country's historic and continuing debate over universal military service. He asks—and answers—such tough questions as, "Do we believe that military service is an obligation of citizenship?" (p. 1). The answer is that we have gone back and forth but that finally, universal service and training "are alternatives that we have elected not to adopt" (p. 171). Again, "To what degree has our military force been manned by citizen-soldiers?" (p. 1). It has been so manned in dramatically varying degrees at different points in our history. I know of no other volume that traces in so comprehensive and

concise a manner this country's vexed and ambiguous approach to citizenship obligation and military service.

Because the contemporary debate has been shaped by enormous social trends, including those that brought more women into the military, and because, over time, social benefits have become "separated from the notion of service," citizenship itself is "reflected more in rights than responsibilities" (p. 3). Military service is more and more a job and the military increasingly structures itself in conformity to a labor market, managerial model. Stiehm's "I'll fight if I want to and if I don't, I won't" military women can therefore be seen as part of a much wider trend toward rationalization and individualization in all spheres of U.S. life, including the military. Segal believes there are genuine problems that we, as a society, have thus far managed to avoid confronting, including, for example, the long-range implications "of the concept of wholly voluntary military service, driven by self-interest rather than citizenship obligation" (p. 15).

We have embarked on a major social experiment. With the creation of the All Volunteer Force (AVF), this country is attempting to "maintain a large standing peacetime force over a long period of time on the basis of voluntary military enlistments" (p. 152). Citizenship and obligation to country, including military duty, have been successfully prised apart (at least in the short term). Segal suggests that we need to think more critically and knowingly than we have about citizenship in general as well as military service in particular. Stiehm helps to fill in critical details on the story and structure of women's involvement in the AVF. But her discussion often gets bogged down in rather technical details and seems lacking a strong, animating vision. In addition, the suggestion that military planners might "give women the Air Force" (p. 6) and the musings with which Stiehm concludes—"How would society be different if people could not be killed—if they either lived forever or died at an appointed age, but never died before or after that age? And how would society be different if men also gave birth and nurtured infants? And would thinking beyond dichotomies be more likely if there were three sexes (or four or five) instead of two?" (p. 241)—take us very far afield indeed, into the realm of what philosophers call "science fiction examples"

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that don't seem to bear directly on the subject matter of her book.

JEAN BETHKE ELSHTAIN

Vanderbilt University

The Coalitional Presidency. By Lester G. Seligman and Cary R. Covington. Chicago: Brooks-Cole, 1989. 191p. \$25.00 cloth.

In 1960 Richard E. Neustadt argued that coalition building was key to the effective exercise of presidential power. Now, almost 30 years later, with demands on the presidency having increased and the linkage between president, party, and public officials having weakened, Lester G. Seligman and Cary R. Covington reiterate the importance of coalition building for a successful presidency. That success, they contend, "depends on the extent to which the presidential core governing coalition constitutes a working majority in each relevant policy-making and implementing arena and how it maintains integrity across the issues on the president's agenda (p. 13).

In making a case for their thesis, the authors begin with the dilemma that every contemporary president faces—how to provide political, policy, and administrative leadership in a polity that divides power, deemphasizes partisanship, and personalizes the quest for office and in a government that represents differing and often competing constituencies.

Noting changes in the electoral process, they claim that it is more difficult today to convert electoral coalitions into governing ones. These difficulties are aggravated by alterations in presidential-congressional relationships, alterations brought on by "(1) the rise of the 'entrepreneurial' congressional representation, (2) the decline in presidential coattails, and (3) changes in the congressional organization that have increased the autonomy of individual members, further undermining its authority structure" (p. 72). Dominating Congress under these circumstances, much less achieving policy objectives, has become a more difficult, time-consuming, and strategic task for presidents. It necessitates a more orchestrated effort, one that involves the White House in policy-making, promoting, and liaison activities. Approximately one-third of those who

work in the White House are engaged in these critical coalitional-building or coalition-maintaining functions.

The public dimensions of these coalitional tasks are enormous! Not only must presidents try to affect the general environment in which they operate, but their office must create specific policy majorities on an issue-by-issue basis. To do this several constituencies must be dealt with simultaneously. Public approval must be maintained at reasonably high levels; majority coalitions must be built by adding to core supporters those who can be induced to follow the president's lead on specific priorities; and interest group alliances must be formed and reformed. These efforts involve public relations on a grand scale. The White House must influence the media's agenda by coordinating informational releases, using the presidency as a bully pulpit and comforter pulpit as well as serving the representational, policy, and political needs of special interest groups. Moreover, the entire effort must channel the public response toward politically relevant officials, including those within the executive branch.

Presidents cannot even count on their own subordinates to be subordinate to their interests, desires, and policy goals. Given the traditional triangular relationship among Congress, interest groups, and the nonpresidential bureaucracy; the long-standing autonomy of much of the executive branch substructure; and the bureaucracy's tendency and capacity to resist intrusions that upset ongoing perceptions, procedures, and personal relationships—given these, the White House has its work cut out for it even on its own turf. Yet the incentives for focusing more attention and resources on this turf have increased with the parochialism of Congress, its special interest orientation, and (more often than not) the partisan and institutional hostility that greets many presidential initiatives.

Seligman and Covington see little alternative to persistent coalition building by the presidents and their staff. With no enduring partisan majorities, no ready compliance to commands, and little policy consensus, presidents must promote their own policy and mobilize their own supporters if they are to be successful. No one else, no other institution or organization, can or will do it for them most of the time. The only other options, they claim—

exercising characteristic leadership or governing by crisis—are short-term imperatives, not long-term solutions. Besides, their frequent use is inconsistent with the principles and, hopefully, the operations of a democratic society.

Nor are the authors upset by the prospect of a coalitional presidency, of presidents “who are sometimes active sometimes inactive depending on their ability to organize relevant constituencies into coalition” (p. 162). They see this prospect as consistent with the constitutional intent and design. Were the framers alive today, Seligman and Covington contend, they would have the last laugh. Perhaps so, although the president’s roles and expectations are considerably greater than the ones they envisioned.

Despite their insipid and more or less irrelevant conclusion (given the theme and general thrust of their work), Seligman and Covington have provided a coherent, intelligent synthesis and analysis of the dilemmas of the contemporary presidency. The value of their effort lies less in the new information they provide about the president’s leadership problems than in how they conceptualize the solution. Forget about traditional notions of a stable, two-party system or even of presidential and congressional parties. The name of the game is clearly coalitional politics. The quicker presidents learn to play this game, the better off they (and the country) will be.

STEPHEN J. WAYNE

Georgetown University

Wrong Number: The Breakup of AT&T. By Alan Stone. New York: Basic Books, 1989. 381p. \$21.95.

Ultimately, concludes Alan Stone, the breakup of AT&T, the world’s most successful corporation, “happened not because of major failings in technological or economic performance but because of politics” (p. 4). In this comprehensive and highly readable work, Stone explains the historical and contemporary contexts of those politics from the disputed beginnings of the telephone to the breakup of AT&T and beyond.

Stone presents the rise and fall of AT&T as a series of three distinct, yet inseparably related, phases of public policy decision making. The

first of these phases began with the invention of the telephone and disputes over its true inventor and legitimate patent holder. Although born in an era of laissez-faire economics, the telephone, like other public services, was and continued to be highly regulated. The major role of regulator during this period was initially played by the state public utility commissions and, to a lesser degree, the Interstate Commerce Commission, with the latter’s role transferred to the newly created Federal Communications Commission (FCC) in 1934. Characterized by the development of a single, integrated network managed by AT&T, this period saw the realization of the regulated network manager system that would remain fundamentally intact until the 1984 divestiture. Despite a New Deal philosophy frequently antagonistic to big business, the general assumption of that period was that the system would remain intact, given that “rates and profits were reasonable, technological progress was remarkable, and virtually every other aspect of performance was considered superior” (pp. 14–15). To the extent that conditions were below standard, the public utility commissions forced improvements—often against the will of the companies involved.

The second phase of this telephone policy constituted a dramatic change from the first. Beginning earlier but culminating in the late 1960s, this phase was characterized by an increased regulatory role by the FCC, due in great part to the growth of federal power and jurisdiction after the New Deal; increased judicial activism in telecommunications policy; and increased technological advances in computers, satellites, microwave relay, and coaxial cable. During this period, the federal government filed antitrust actions challenging the vertical relationship between AT&T and its major supplier, Western Electric, while the FCC’s definition of the “public interest” changed to accommodate new demands from AT&T competitors and from other interests wishing to utilize these competing services. Most notable among competitors was Microwave Communications Incorporated (MCI), which initially sought to supplement services and products provided by AT&T but later became a full-scale competitor to the long-distance service provided by AT&T. Perhaps most notable among interests eager to take advantage of services offered by AT&T competitors are IBM

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and the American Petroleum Institute, which ultimately saw communications as a highly central part of their continued operations and continued existence. Supported by sympathetic staffers and commissioners on the FCC, an activist judiciary, and a general philosophy of opposition to monopoly in the United States, these interests were successful in convincing the major policy makers that their preferences were in the public interest.

The policy muddle that resulted from the second phase's various, often conflictual FCC pronouncements inexorably led to a third phase beginning in 1974 with MCI's private antitrust suit against AT&T and the Sherman Act suit filed by the Department of Justice later that same year. The result of this shift in strategy by MCI and the government was to decrease the ability of the FCC to regulate the industry while concomitantly increasing the power of the courts and activist judges. The 1982 divestiture settlement and the 1 January 1984 dissolution of the old Bell system as it had previously existed terminated phase 3 in the AT&T saga.

Stone's work has a great deal to offer students of U.S. politics, public policy, public law, and public administration. He effectively refutes the notion that regulatory agencies are necessarily captured by the regulated. Clearly, the many adverse decisions by the FCC during the second phase indicate that AT&T's economic size and power did not translate into political power. Indeed, to a great extent, its power and position worked to AT&T's disadvantage before the commission and Congress. Secondly, as Donald L. Horowitz has noted in *The Courts and Social Policy* (1977), the courts, due to their unique design among political institutions, are often incapable, as they were in the AT&T litigation, of making general policy decisions (especially in so complex and interconnected a field as this). Thirdly, Stone illustrates how "muddling through" with incremental decisions that fail to take account of one another often results in major, unforeseen impacts when they are aggregated as a policy whole. Similarly, such decisions illustrate the tactics of groups such as MCI to attack collaterally (and succeed) where a frontal assault would most assuredly have led to defeat. Like incremental budgeting, whereby a single appropriation often leads to permanent and ever-increasing budgets, the

camel's nose in the tent proved to be a successful strategy for those intent on skimming the cream from AT&T's decades of solid management practices. Finally, Stone provides with a lawyer's skill and a political scientist's insight (he is both) excellent discussion and analyses of the major FCC decisions and court cases that shaped telecommunications policy in the United States over the course of the twentieth century.

Frequently, however, Stone seems to abjure the role of social scientist by adopting an almost sycophantic pose toward AT&T and its accomplishments, which (admittedly) are many. Yet one would like to see the other side of this policy coin in Stone's work, that is, interviews or other materials from the FCC commissioners and staff or MCI, IBM, or American Petroleum Institute executives relating to their motives and perceptions on these various policy disputes. What, for example, were the FCC commissioners' own perceptions of their decisions? Indeed, with few exceptions the membership of the FCC at each critical juncture is not discussed. Who are these commissioners, what are their respective backgrounds and ideologies, and which president appointed them? In other words, other than generalized notions about monopoly and the benefits of competition, what was it about the FCC commissioners in each of these periods that led them to adopt a new definition of the "public interest"? Public law research and the recent actions of Reagan appointees to the FCC would surely indicate that the particular individuals sitting on the FCC during each of Stone's three phases were important and decisive to the decisions made by the commission. Likewise, from a political and social science perspective it would have been beneficial if this work had more fully analyzed the AT&T divestiture within the context of recent public policy literature on policy success-failure and policy initiation and implementation.

Overall, however, Stone's work is a monumental one that explains the unexplainable: Why was the world's most successful corporation disassembled to achieve putative benefits that were tenuous at best and disadvantageous at worst to most consumers? From the invention of the telephone to the still-unknown consequences of divestiture, Stone explains in comprehensive detail the complex political,

social, economic, and technological environment that led to what Charles Wohlsetter of the Contel Corporation describes as "probably the single most stupid and damaging decision that has ever been made in the history of business in any country." Of the many volumes written on the breakup of AT&T, perhaps no other is as comprehensive, detailed, and totally fascinating as *Wrong Number*.

MICHAEL W. BOWERS

University of Nevada, Las Vegas

Seats and Votes: The Effects and Determinants of Electoral Systems. By Rein Taagepera and Matthew Soberg Shugart. New Haven, CT: Yale University Press, 1989. 292p. \$27.50.

Representation, or the translation of the desires of citizens into policy, occupies the daily attention of most political scientists, whether their interests be positive or normative. Taagepera and Shugart have written an ambitious treatise on the implications of the various institutional mechanisms for translating votes into seats in legislative bodies. Their ambitions take at least two forms: (1) to gather together in one place, and rather considerably extend, all that is known about the relations between seats and votes; and (2) to conduct a seminal exercise in the philosophy of social science. In the first enterprise they largely succeed; but the execution of the second is so intrusive that the book is ultimately divided and distracted, altogether less than the sum of its mostly excellent parts.

Taagepera and Shugart, like numerous others, have sought in various earlier forums to investigate, formalize, and generalize the relation between seats and votes. If the goal is explaining representation in democracies, one would hope that attention would be paid both to how votes are translated into seats and how the institutions of the legislature then translate seats into political will (i.e., Is there a com-

mittee system? What size majority is required to enact legislation? How do the rules of the legislative process advantage or disadvantage certain groups?). This is not to trivialize the relation between seats and votes; but such a focus does ignore how seats become policy. The link between individual votes and policy is the justification for the whole enterprise. Normally, this would not be a very damning criticism, since no one could hope to cover a theoretical and empirical comparative study of the voting and legislative institutions of the democracies in a single book. But the grand claims that Taagepera and Shugart make ("The study of politics may yet become a science in the sense of a cumulation and interlinkage of quantitatively testable theory. And seats and votes may be the Rosetta stone" [p. 246]) do merit circumspection: Why spend so much time on so narrow a question?

Still, Taagepera and Shugart have created a genuine contribution to knowledge. The work is organized most felicitously, and the writing and notation are for the most part accessible for the minimally numerate reader. The authors go to great lengths to explain the features of electoral systems that influence the translation from votes to seats and give numerous and tremendously useful examples as illustrations. The most useful "example" is an extended, chapter-length comparison between New Zealand and Finland, two countries that (the authors point out) are at opposite ends of the earth and of the spectrum of democratic institutional arrangements. This is followed by a careful and extensive review of the history of the study of electoral systems, tracing both its chronological and intellectual development.

The largest section of the book is that devoted to the empirical analysis of electoral systems. Taagepera and Shugart here review and extend their own and others' work on the effective number of parties, district magnitude, and Duverger's Law. In each case they provide a means of defining and precisely measuring concepts that are on their face vague and subject to contradictory interpretation. The three chapters ending this section constitute a mostly new and, I believe, extremely important contribution to the theory of electoral systems. The authors present and prove a simple, purely theoretical, theorem demonstrating that the so-called cube root law of assembly sizes

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(the number of seats in national assemblies tends to be approximately the cube root of the population of that nation) has a rational basis. This size minimizes the sum of external (constituent) and internal (legislative) communications costs. While this is an interesting first step, the emphasis on communications costs within legislatures painfully highlights the authors' narrow and limited theoretical perspective on legislatures as groups of equals, in contrast to the U.S. legislature (one of their "confirmations" of the theory), which has an elaborate system of rules and a committee structure to mitigate the costs and control the flow of communication.

Some other trifling mistakes aside ("Slaves counted as *half-persons* for the purpose of allocating congressional seats to the states" [p. 18, emphasis mine]), Taagepera and Shugart have on the whole written a useful book. Unfortunately, the last chapter, rather than summarizing the undeniable contribution they have made in one subfield of the discipline, is a broad discourse on the philosophy of science in which Taagepera and Shugart play Moses to political science's Jews in Egypt: "For the first time, we have in political science a quantitatively linked construct reminiscent of the structure of [physics]" (p. 246). This claim seems incredible for a book whose references indicate nothing that has been written in rational or formal theory since Downs (1957). My advice is to read the last chapter separately, as it has little to do with the rest of the work.

In sum, *Seats and Votes* will be a widely recognized and valued piece of work; but its methods are unlikely to serve as a new Rosetta stone. We must ultimately recognize it as an increment in a much older program of developing a political science. We can applaud the authors' optimism, but their words hark back to a similar assertion now two centuries old: "The science of politics, like most other sciences, has received great improvement. The efficacy of various principles is now well understood, which were either not known at all, or imperfectly known to the ancients. . . . The introduction of legislative balances and checks . . . the representation of the people in the legislature by deputies of their own election: these are wholly new discoveries, or have made their principal progress towards perfection in modern times" (Alexander Hamilton in *The Federalist* 9 [1788]). *Seats and Votes* is a

step in that march, but hardly the journey's end.

MICHAEL C. MUNGER

University of Texas

The Energy Crisis and the American Political Economy. By Franklin Tugwell. Stanford: Stanford University Press, 1988. 294p. \$27.50.

Shale Barrel Politics: Energy and Legislative Leadership. By Eric M. Uslaner. Stanford: Stanford University Press, 1989. 241p. \$27.50.

The oil shocks of the 1970s disturbed U.S. political institutions to their foundations. The ensuing shortages and economic upheavals produced a decade of anxiety for the average citizen looking to Washington for insulation from such shocks. They also brought a decade of insecurity for politicians who charted U.S. economic and national security policy. These two volumes provide complementary treatments of how U.S. national institutions responded to the energy crisis and its demise through the 1980s. The Tugwell book evaluates energy policy from the broad perspective; the Uslaner book evaluates it as a problem of legislative decision making. Both offer bleak assessments of the capacity for U.S. institutions to respond coherently to future shocks that will almost certainly occur. However, neither offers a viable prescription for reform of energy policy making.

Tugwell focuses on energy policy regimes in broad overview. The first seven chapters provide a detailed history of policy making for coal, oil, natural gas, and electricity, and the response of these regimes to the oil shocks. Intertwined in this analysis are explanations couched in political economics, interest group theory, theories of regulation, and subsystems theory. Tugwell argues, as have others, that energy systems are adaptive entities responding to incentives and constraints imposed by the interplay of politics and markets. Energy producers, wanting to avoid the uncertainties of competition, seek market stabilization through "proprietary" regulation or other means. Market structure determines producer ability to acquire stable markets. So, too, do

consumer preferences. Energy consumers are an often-diffuse group, cycling between periods of latent and intense interest in energy issues. Before 1974 they were content to allow inefficient regulated markets as developing production technology kept energy prices low. After 1974 these conditions changed, resulting first in "compensatory" regulation during the Nixon, Ford, and Carter administrations and later in deregulation during the Reagan administration.

The last four chapters of the Tugwell book are a critique of U.S. institutional capacity to respond coherently to future oil shocks. Tugwell argues that government responses to past crises were hostage to pluralist forces. The cleavage between energy producers and energy consumers produced policies working at cross-purposes. The rift failed to promote national interests, namely, minimizing wealth losses to oil-producing countries and protecting national security. Some responses were consistent with national goals (e.g., mandatory highway speeds, fuel efficiency requirements, building standards); others were not (e.g., price controls, discriminatory pricing, and the windfall profits tax), actually increasing the demand for foreign oil and decreasing national security. Tugwell concludes from all this that pluralist democracy, with its intrinsic resistance to change and tendency toward compromise outcomes, is unlikely to produce coherent responses to future crises. He suggests increased economic planning to address this problem.

Uslaner takes as a starting point for his study that the United States has no cohesive energy policy. He attributes this to a failure of cooperation within U.S. legislative institutions. The key question of the book is why Congress has remained deadlocked for so long over energy issues. Two competing explanations are considered: a structure-induced equilibrium biasing that institution against change and congressional preferences that preclude action. Uslaner supports the latter explanation, asserting that members of Congress do not decide energy issues because their constituencies constrain them from doing so. Moreover, congressional structure is endogenous to preferences for energy issues, so institutional reforms are unlikely in the future.

Of course, Uslaner does not deny that legislative structure is important to policy outcomes. Rather, he distinguishes between traditional pork barrel politics in which structure dominates and what he dubs "shale barrel" politics in which preferences overwhelm structure. Pork barrel politics involves the distribution of *benefits* to congressional constituencies, while shale barrel politics involves the distribution of *costs*. Uslaner argues, using rudimentary game theory and empirical analyses, that the politics of costs distribution differs fundamentally from the politics of benefits distribution. Benefits induce coalitions of cooperative minorities, while costs produce "coalitions of destructive minorities." To illustrate this phenomenon, Uslaner presents three energy policy case studies, each showing the presence of destructive coalitions of minorities and resulting policy paralysis. He also compares these with the Canadian case, where energy policy making was successful. He asserts—unconvincingly in my opinion—that the difference between the U.S. and Canadian cases was cultural, not the result of different institutional frameworks. Still, Uslaner makes a persuasive argument for the economy of external incentives as important determinants of cooperation-noncooperation in a legislative setting.

Both books are useful reading for students of U.S. energy policy, but the Uslaner book should appeal to a broader audience. Tugwell locates current energy policy in the context of historical development, which enables predicting future responses to oil shocks. Uslaner does the same but in an important sense contributes more than just an increased understanding of energy policy. His case study also provides important material for developing theories of cooperation-noncooperation in political institutions. Viewed in this light, *Shale Barrel Politics* should be of interest to a range of political science subfields. Students of research methods, legislative politics, decision-making theory, and the neoinstitutionalism will all find occasional plums to make the reading worthwhile.

B. DAN WOOD

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The Ecology of City Policymaking. By Robert J. Waste. New York: Oxford University Press, 1989. 219p. \$32.00 cloth, \$15.95 paper.

From time to time, studies of urban politics have been published that gain the status of "classic works" in contributing to our knowledge of the urban governmental and political world. Such works would include, of course, Dahl's *Who Governs?* (1961), Banfield and Wilson's *City Politics* (1963), and Peterson's *City Limits* (1981). I would like to wager that Robert J. Waste's *The Ecology of City Policymaking* will also gain this coveted status. In his discussion of city policy making, Waste has done a masterful job in weaving together theoretical and case study materials in a well organized, integrated, and written volume.

Concerning city policy making, his argument is, "Policymaking in American cities is best viewed as the result of the policy cycle of a city interacting with, and affected by, ten internal and external variables in accordance with routinized laws that govern the behavior of local governmental bodies and assign advantages and disadvantages to various players and policy alternatives" (p. 4). Stated in a more specific manner, he argues that the following 10 ecological variables—age, locale, growth process and the role of growth, local political culture and ethos, personalities of key elected and appointed policy makers, presence or absence of political scandals or reform efforts, types of policy conflicts, types of policies enacted, presence and strength of regulatory activity, and exogenous factors involving intergovernmental relations—along with the basic, city policy life-style constitute the ecology, or environment, of city policy making.

To a significant extent, the basic strength of the volume is based on the author's ability to build, integrate, and expand upon existing theoretical concepts and develop refined conceptual models. For instance, in formulating a comprehensive model of the city policy process he utilizes as his intellectual foundation Bryan Jones's policy cycle construct set forth in *Governing Urban America* (1983) but modifies this construct with concepts drawn from the agenda-setting policy model developed by Roger Cobb and Charles Elder in their *Participation in American Politics* (1983).

Another example of Waste's theoretical synthesizing involves his application of Anthony Downs's eight-stage life cycle of administrative bureaus, set forth in *Inside Bureaucracy* (1959) to explain the evolutionary stages of city councils. Although the author's empirical data base is weak—interviews conducted with several elected and nonelected political activists in 15 recently (1968–74) incorporated council-manager-administered California cities reinforced by a reading of newspaper accounts—he does develop a rather intriguing theoretical model of council evolution. Yet it is "fair game" to posit the degree to which this model would be impinged upon (and hence require modification) by various forms of local government.

Given the overall high quality of this work, Waste lets the reader down in the final chapter, where he argues that due to the inherent fragmentary character of policy making, the city is a weak policy-making unit, especially given the multitude of socioeconomic problems confronting U.S. localities. On this point, I am afraid that the author's brush is too wide, as some cities have proved to be more resourceful than others in dealing with their urban condition. And even if we grant him his basic point that cities are weak policy-making units, he leaves us intellectually hanging with no offered remedy.

Waste also includes in his volume an informative set of notes, an excellent glossary, and a useful bibliography that further secure that *The Ecology of City Policymaking* is an excellent addition to the literature on the city policy-making process and one that will stand the test of time.

NELSON WIKSTROM

Virginia Commonwealth University

Mr. Justice Black and His Critics. By Tinsley E. Yarbrough. Durham, NC: Duke University Press, 1989. 323p. \$45.00.

Few would deny that Hugo Lafayette Black must be counted as one of the towering figures to have occupied a seat on the U.S. Supreme Court during this century, and his influence upon modern constitutional law has been substantial. Yet as President Franklin Roosevelt's first appointment to the supreme bench in

1937, Black was initially tarnished by his ties to the Ku Klux Klan; and his sparse legal training appeared to make him ill prepared to assume a commanding role on the Court. His unstinting support for civil rights for blacks, as well as civil liberties generally, soon dissolved the doubts raised by his Klan ties, however; and despite his lack of apparent preparation for service on the Court, Black rapidly assumed a place among the ranks of the more influential justices of the Roosevelt Court. And Hugo Black possessed another attribute normally requisite to being numbered among the most influential of the justices to have served on the Court—longevity. He served almost thirty-four years, and perhaps few if any other justices who have ever served on the Court lived to see more constitutional positions initially advanced in dissent ultimately adopted by a majority of the Court than he did.

Tinsley Yarbrough's volume is a systematic analysis of Hugo Black's constitutional philosophy and the positions he advocated regarding the major constitutional issues addressed by the Supreme Court during his service on the bench. Since Yarbrough focuses on the theoretical underpinnings of Black's judicial philosophy, his volume may be usefully supplemented by the reader with Hugo Black, Jr.'s *My Father* (1975), which furnishes useful insights into the senior Black's personality; Virginia Van der Veer Hamilton's *Hugo Black* (1972), which deals with Black's political career in Alabama until his appointment to the Supreme Court; and Gerald T. Dunne's *Hugo Black and the Judicial Revolution* (1977), a historical overview of the work of the Court during Black's tenure.

Hugo Black's jurisprudence has been a research interest of Yarbrough for more than 20 years; and that prolonged concentration of research is clearly reflected in this volume, since it presents in a highly readable form the most systematic analysis of Black's constitutional philosophy that has yet appeared. In addition, Yarbrough thoroughly examines the arguments of the most prominent critics who assailed Black's constitutional positions as he expounded them over the years. Black's contention that the Fourteenth Amendment was intended totally to incorporate the Bill of Rights (which, Yarbrough somewhat surprisingly concludes, Black believed was ultimately accomplished by the Privileges and Immunities

Clause rather than the Due Process Clause); his absolutist approach to the interpretation of the First Amendment guarantees; and his rather restrictive construction of the Equal Protection Clause as well as his rather ambivalent attitude toward the Fourth Amendment and the source of the exclusionary rule—these are all thoroughly explored along with the criticisms that have been directed by the Black philosophy of constitutionalism.

Hugo Black, Yarbrough concludes, was a legal positivist who, like most positivists, distinguished between law and morals, assigned the principal lawmaking function to legislatures rather than courts, based the interpretation of the law upon the intent of the law's framers and highly valued both clarity and consistency in the law. Black, Yarbrough contends, "was striving for constitutional constructions which were compatible with plausible readings of language and history, and thus consistent with his conception of judicial review, yet restrictive of judicial choice, and thus compatible with democratic principles." "Taken together with his total incorporation thesis and literal, generally liberal readings of specific Bill of Rights safeguards," Yarbrough concludes, "his constructions of due process and the other open-ended clauses formed a jurisprudence which arguably struck a workable, if imperfect, balance between the Constitution's critical role as a meaningful restraint on governmental power and the equally important majoritarian institutions the document also embodies" (pp. 260-61).

As one of those who believes that Hugo Black's interpretist approach to constitutional law was overly simplistic and failed to achieve the success that Yarbrough claims for it, I confess that I remain in the camp of Black's critics. It is a monument to the excellence of his analysis and arguments, however, that if I could be convinced otherwise, Tinsley Yarbrough's volume, more than any other exposition of Hugo Black's jurisprudence, would have accomplished that objective.

RICHARD C. CORTNER

University of Arizona

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COMPARATIVE POLITICS

Beyond Aztlan: Ethnic Autonomy in Comparative Perspective. By Mario Barrera. New York: Praeger, 1988. 209p. \$34.85.

The struggle for equality and recognition of Latinos in the United States has been long and arduous. It is a history of a territorial minority characterized by an institutionalized system of discrimination and inequality. The Latino response to racism and exploitation has been to unite, organize, and gain control of, their communities and the institutions that affect them. The supposition has been that community control will ultimately give them "more equitable social policies and greater economic and political equality," which will in turn safeguard the integrity of their cultural distinctiveness. Yet, Mario Barrera, a long-time supporter of these causes, argues that the goal of ethnic nationalism has been ambiguous and has had limited results. Because of the unclear and uncharted course of political action, the author contends that no clear statement of the relationship between Chicanos and the political system ever emerged.

In part 1 of a two-part book, Barrera convincingly argues that two basic goals dominate Latino political history: equality and community. *Equality* is defined in economic, social, and political terms between Latinos and Anglos (non-Latinos) and *community* is defined as "the maintenance of a cohesive and culturally distinct communal identity" (p. 4). Key to the purpose of this work is the relationship between these two fundamental goals. In Barrera's words, "The focus continues to be on achieving greater economic mobility for Mexican Americans, and yet the very successes of that effort . . . serve to undercut the distinctive collective identity that has so frequently been articulated as a political goal" (p. 5). In Latino leadership and organizational politics, either one is stressed over the other, or both goals are emphasized at the same time. This dilemma represents major consequences and contradictions to Latino empowerment.

In part 2, Barrera charts a loose course for ethnic autonomy by embarking on a research

mission abroad in search of other multiethnic societies to study how governments have dealt with minority-majority relations. Through a "long process" that is not described the author focuses on a group of four countries: Canada, China, Switzerland, and Nicaragua. These four countries do have some things in common: all have multiethnic societies, all provide a structural arrangement to recognize and protect the rights of various ethnic groups, all have constitutions and laws that declare the multicultural-lingual nature of society, and all have evolved a system of governance whereby ethnic minorities have achieved some political control over their community and historic territory. Because of its long involvement with ethnic minorities, each of these countries can offer different avenues whereby Latinos can ultimately chart a new course of action.

Barrera's concluding remarks are the most comparative since he draws several implications for the United States from these four distinct societies. Moreover, the author offers the distant possibility of achieving "a reconciliation of the goals of community and equality in this country, achieving social justice without sacrificing cultural diversity" (p. 7). It is understood that the author's arguments are long-term goals that require a thorough and major reorientation of majority-minority relations in the United States. In some ways, this reorientation can be characterized as the reversal of the contemporary English-only movement.

To "go comparative" is intellectually sound and a necessity. In the main, Latino literature has been either localistic or regionalistic, isolationist, and, for all practical purposes, non-comparative. Only a few comparative studies, such as Walker Conner's edited *Mexican Americans in Comparative Perspective* (1985), offer the reader the grand opportunity to extrapolate various theoretical frameworks on ethnic politics from other multiethnic societies. Yet one fundamental question remains, that of the process and substance of comparative studies. In this study, the author successfully documents the goals and dilemmas of Latinos

and argues for new directions. It is his comparative initiatives that are troublesome. Although this is not a book on methodology, the rigor of his "long process" to arrive at a comparative analysis between Chicanos and other ethnic groups is not present. No one could argue that societies throughout the world provide a wealth of opportunities to learn and compare. Nevertheless, descriptive studies have marginal, if any, explanatory power. Greater depth, more focused ideas, and more rigorous analytical frameworks are a must if we are to apply some of these ideas adequately. However, if one takes this study as one step toward more comparative studies and if one looks at a long-term political effort to reorient Latino political goals (as he urges us to do), the book serves its purpose well. In spite of its shortcomings, Barrera has made an extraordinary effort to continue a process of categorizing the political experiences of other ethnic groups comparatively in search for possible answers to contemporary Latino goals and dilemmas.

ROBERTO E. VILLARREAL

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Japanese Government Leadership and Management. By Charles F. Bingman. New York: St. Martin's, 1989. 165p. \$49.95.

After World War II, Japan, under the American Occupation Authority, completely restructured the institutions of its national, prefectural, and municipal governments, created a public infrastructure capable of supporting economic growth, and deployed a whole new array of vastly improved educational and social service programs for its citizens. This book aims to inform readers how the postwar Japanese government works.

The author's basic theme is that the success of Japan's postwar economic and social revolution should be credited to the three "rings of power" that rule Japan: the leaders of the ruling Liberal Democratic party (LDP), the Diet, and the career bureaucrats who have headed the various ministries and agencies of the national government.

To support this theme, the author deals with many aspects of Japanese public administra-

tion, working his way "from the top down" after an introductory chapter treating the basic structure and theory of Japanese government. Of particular interest are his discussions of policy regulation and administrative guidance (chap. 8) and the culture of the civil service (chap. 12). An intriguing case study shows the intertwining of government and industry in the area of transportation. Another case study on the educational system, focusing on the role of the Ministry of Education, is timely, informative, and worth the attention of U.S. readers.

The author, a former deputy associate director of the U.S. Office of Management and Budget and deputy administrator of the Urban Mass Transit Administration, draws on his extensive experience to provide some valuable insights into how the Japanese government manages its public programs. He investigates Japanese administrative structures and processes as a professional administrator rather than a theorist and is more concerned with the way Japanese bureaucrats solve problems than with the nature of their problems. He evaluates administrative coordination from the viewpoint of a manager rather than that of a low-level public employee. Thus, his approach to the subject is legalistic, formal, and decidedly management-oriented.

His conceptual foundation—the notion of the three rings of power, and the idea that career bureaucrats in the government ministries have been the real architects of the postwar economic recovery—is not original. Yung Park, a U.S. professor, offered a similar concept in the 1970s, as did Diet member Koji Kakizawa—though in their view the three rings of power were the bureaucratic leaders, the LDP, and the business community. Eiich Kato, a Japanese professor, also studied the three rings of power in the 1980s. Many Japanese regard the career civil servants as the strongest of these three rings because the leaders of the LDP, the Diet, and the business community (themselves often former bureaucrats) traditionally follow their course. Thus far, events tend to support the author's theme. The real question is whether the economic miracle will continue if the LDP fails to keep its majority in the Diet.

Such a short book cannot cover all the major aspects of the subject, and one has the feeling that it lacks depth and critical analysis. The

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author uses many tables and figures but often fails to cite the source of his information. There are some glaring inaccuracies. For example, the authors says (p. 137) that over half of the successful candidates at the higher civil service entrance examination are graduates of the Tokyo University law faculty; but statistics from the National Personnel Authority (*Jinjiin*) show the figure has fallen from 40% in 1981 to 31% in 1987.

Despite these shortcomings, the book provides a solid introduction to Japanese public administration for English-speaking readers. It is well written, free of professional jargon, and straightforward. It deals clearly and logically with a number of topics, such as administrative guidance, the transportation and education systems, and public enterprise. Moreover, it challenges Westerners' generally negative view of public servants as lazy, rigid, apathetic, unimaginative, and legalistic, instead portraying Japanese bureaucrats as hard-working, dynamic, considerate, and resourceful. Perhaps most important, it signals clearly (as did Woodrow Wilson's famous 1887 article, "Study of Administration") that the time has come for U.S. students of public administration to give serious attention to the highly efficient methods of foreign governments—especially the Japanese government and its public programs, which he calls "a remarkable achievement in public management."

PAUL S. KIM

Gannon University

The USSR: Language and Realities. By Michael Bruchis. Boulder: East European Monographs, 1988. 393p. \$44.95.

The two nouns in the subtitle of this book suggest a general tension that its author explores with specific reference to two areas of policy in the Soviet Union. The first area involves authoritative texts, such as the 1961 and 1986 programs of the Communist party, speeches of various general secretaries, and tracts on party history that pretend to define the nature of the USSR and the challenges that it confronts. The author painstakingly searches out the inconsistencies and fictions found in these materials in order to demon-

strate that the claims of the Soviet authorities with respect to the USSR's constitutional order, its history, the status of national groups within it, and so forth are no more than elaborate exercises in bad faith. The second area concerns languages, specifically those of the non-Russian minorities. Here, the author tries to "prove" that the policy of the Soviet regime has been one of unremitting Russification that has mendaciously robbed the non-Russian minorities of their native languages to the point that their "national languages cannot be considered any more as the main means of communication" (p. 264).

The author is himself an émigré Soviet scholar; and unfortunately, most of his book seems intended to settle old scores with former colleagues. Ironically, although he makes no secret of his contempt for their views, his attacks on them and the political order that they represent are mounted within a discourse that is structurally indistinguishable from that associated with official rhetoric in the *preglasnost* era. The result is a scholasticism that abuses logic in order to reach preconceived conclusions. In the space of two pages, for instance, we learn that the party organizations of the union republics are the "executors of the will of the Kremlin rulers and not of the population of the respective republic" and that the Russian republic does not have its own party organization, precisely because if it did, this national party organization, for reasons unexplained, would act "contrary to the will of the Kremlin bosses" (pp. 88-89). Empirical questions are likewise decided by the exigencies of the author's belief that change in the Soviet system is unidirectional: from bad to worse (pp. 49-50, 371). Accordingly, he contends—sans evidence—that the Stavropol region during Gorbachev's tenure as first secretary of its party committee was as corrupt and mismanaged as any other in the USSR (p. 71) and that previous military interventions in East Europe are "wholeheartedly supported by the current leadership of the CPSU" (p. 372).

Perhaps the most disconcerting passages of this book, however, concern us in the West. Western governments and publics are regarded simply as naive and gullible and therefore easy marks for Kremlin disinformation, of which *perestroika* in all its forms is merely the latest installment. Western scholars whose assessments of Soviet nationalities policy coincide at

some point with the views of the author's former colleagues are portrayed as dupes by simple virtue of that coincidence.

Reading this book renews one's appreciation for the power of discourse. The habits of mind, the ways of looking that *perestroika* promises to alter in the USSR are amply represented in this tome, with black and white reversed. At one remove, then, we get a sense of how deeply ingrained the old ways are and of the scope of a project that would restructure them.

MICHAEL E. URBAN

Auburn University

The Politics of Progress: The Origins and Development of the Commercial Republic 1600-1835. By Hiram Caton. Gainesville: University of Florida Press, 1988. 639p. \$49.00.

The Politics of Progress is an ambitious, solid, and lively work. It is a piece of interpretive scholarship to be much admired in terms of both the tradition into which it fits and its sheer audacity. Hiram Caton does not shy away from tackling a huge topic; and with some difficulties along the way, he makes a noteworthy contribution to our understanding of this critical period in political thinking.

As the subtitle notes and the author states on the initial page, the book is about preindustrial political thought and the associated notion of progress. He ranges back in time to the sixteenth century in Western European thinking and takes us through the U.S. Federalists and Bentham. I found his discussions of the political anthropologies of Hobbes and *The Federalist* quite stimulating. For anyone seeking a rewarding and even-tempered handling of many of the debates over such things as republican theory and Jefferson, for example, Caton's work is an invigorating illustration of scholarship and style.

The discussions assist in furthering our understanding of how economic and political thinking were interwoven within these perspectives. By so engaging us in an examination of the formative steps and stages of liberal thinking, Caton provides us also with a more informed position for reflecting on our more liberal presuppositions.

Caton succeeds, I believe, in making his case that the notion of progress is the thread that essentially strings political economy thoughts together during the fertile 1600-1835 period. He is persuasive in showing how the emergence of the factory system was integrated into the paradigm of moral progress and how the latter became a rationale and justification for the former. Importantly, the author also leaves the reader with an ample basis for reflecting on the reasons why the initial, progress justification for factory organization, for example, has not reaped the intended rewards, at least for many who labor within its grasp.

I liked, and profited from, Caton's highly informative and useful footnotes, with crisp discussion of many different points of view. Perhaps without setting out to do it, Caton has made a strong brief for the traditional, reflective footnote style. Along these lines, I also found the bibliography complete and useful.

While one of the book's virtues rests in Caton's efforts to tie a number of thinkers and movements together, at times this objective becomes a problem. He does lose track of his central theme of progress, especially across the first 243 pages. The book could have profited from further editing. At times, because of these problems, this is a difficult book to manage efficiently.

Notwithstanding what are essentially quibbles, Caton's efforts are to be judged as solid, positive contributions. He has helped us gain ground toward an understanding of the period and provided fresh interpretations of several thinkers within the 1600-1835 span. Of equal significance, Caton has enriched our own contemporary conversations about political economy relationships.

CARL F. PINKELE

Ohio Wesleyan University

Feminism and Politics: A Comparative Perspective. By Joyce Gelb. Berkeley: University of California Press, 1989. 253p. \$30.00.

In the past few years, comparative research on women's politics has flourished. In the search for a more systematic, theoretically oriented explanation of the emergence and

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direction of women's political activity, feminist scholars have recognized the great value of cross-national studies. Joyce Gelb's third book, *Feminism and Politics: A Comparative Perspective*, is the most recent contribution to this growing literature.

The book is a comparison of feminist movements and new public policies for women in the United States, Britain, and Sweden. Gelb relies on the large secondary literatures on these countries (including material from her previous book with Marian Palley, *Women and Public Policies* [1987]) as well as her own extensive interviews with women journalists, politicians, and academics abroad. Drawing on the "political opportunity structure" approach to social movements, Gelb argues that cross-national variations in political institutions, alignments, and ideology have "patterned" the development, organization, strategy, and goals of feminist movements in each country. In turn, the character of each movement (mediated by its institutional, political, and ideological context) has shaped policy outcomes. Gelb concludes that "gender-based women's groups that are separate from institutionalized interests such as parties and unions are more likely to develop independent strategies and political agendas of their own choosing, thus permitting greater political impact" (p. 2).

The heart of the book consists of cross-national comparisons of three different models of women's political participation: the "interest group feminism" of the United States; the "ideological," or "left-wing," feminism of Britain; and the "state equality" of Sweden. These terms refer to the character of feminist activism in each country: the highly organized, coalition-oriented U.S. movement; the organizationally fragmented and ideologically radical movement in Britain; and the virtual absence of autonomous feminist activity in Sweden. Gelb examines the relationship of each of these forms of activism to established vehicles of interest representation in each country. She also explores the extent of feminists' influence in their country's administrative and policy-making process. She finds that although the parties and unions in Britain and Sweden formally support gender equality (though in varying degrees), these issues are frequently ignored in practice due to the absence or weakness of pressure from independent

women's groups. The closed and insulated legislative and administrative process in Britain and the constraining impact of Swedish corporatism further limit feminist political influence in these countries. By contrast, the institutional openness of the political system in the United States, as well as the weakness of U.S. parties and unions, has both stimulated autonomous feminist political activity and enhanced its effectiveness.

In concluding her book, Gelb evaluates the more general success of each model of feminist activism. She focuses on each movement's impact on (1) changed policy outcomes and (2) "group and societal consciousness as well as the legitimization of group goals" (p. 186) and relies on a range of attitudinal and economic data to support her claims. While acknowledging that feminist activity in each country has succeeded in different ways, Gelb argues that the U.S. feminist movement has had a far more profound impact on U.S. society than its counterparts in Britain and Sweden. Unlike developments in Britain and Sweden, the movement here has grown in size and heterogeneity; there is widespread support for its goals; it has won effective enforcement of sex discrimination laws; and U.S. women have moved rapidly into technical and professional occupations (though the argument that this is due to the feminist movement or even to strict enforcement of sex discrimination laws is unpersuasive). Although Swedish women benefit from a variety of policies that facilitate their labor force participation (and that are the envy of U.S. women), general labor force policy of this kind is not good enough, Gelb insists, because it has done little to change sex roles.

This book is plagued by some major theoretical problems. First, like others working with the political opportunity structure approach, Gelb defines this structure so broadly that it encompasses almost all factors that might influence the character and impact of a social movement (e.g., the nature of the central government, the system of interest representation, political parties, unions, ideology, and political culture). Such broad theoretical frameworks, with their multiplicity of independent variables, do little to expand our understanding of the constraints on, and possibilities for, feminist activism. Although most of Gelb's analysis focuses primarily on differences in institutional factors, she does not

press this point consistently enough or explicitly enough to get much analytical mileage out of it. One is left with the feeling that the cross-national comparisons are insufficiently systematic, at least partly because the argument lacks focus.

Second, the book relies implicitly, though heavily, on a pluralist view of politics and policy making, an approach that has been under heavy fire for some years. Indeed, this view is the core assumption of her argument that the U.S. model of feminist activity has been most successful. But the legislative and administrative histories she provides are too thin for the reader to be entirely confident that the full story is being told. As her own (and others') material on Sweden suggests, policies enormously helpful to women can be initiated in the absence of an organized feminist movement. Her argument would have been considerably strengthened if she had either explicitly considered alternative theoretical perspectives or had fine-tuned her argument to focus on specific kinds of policies most likely to result from the activity of an autonomous feminist movement. This latter strategy would have allowed her to highlight her very interesting but insufficiently developed point concerning the seemingly paradoxical Swedish case. Gelb argues that in the absence of organizations articulating women's interests from a feminist perspective, Swedish women's *gender-specific* concerns have been obscured by the emphasis of an economically based, sex-neutral definition of equality. This is an enormously important and original point, rich with theoretical possibilities; but it is not pursued far enough to rescue the book.

Finally, the book's theoretical significance is further diminished by its rather limited focus on women (biological females) rather than gender. Contrary to recent usage, these terms are not interchangeable. Properly understood, *gender* refers to the specific system of social relations between the sexes, not just to women. Framed as a question about the origin and organization of inequalities of power, the study of gender can open an array of exciting new theoretical possibilities (as it has in history, anthropology, and literary studies). But like almost all feminist scholarship in political science, this book situates itself within the conventional categories of the discipline and uses women as a "case study" to examine

some general political phenomenon (in this case, the relationship between social movements, the political opportunity structure, and policy outcomes). Although this approach has generated some good research over the years, it is now time for feminist political scientists to begin developing a distinct research agenda focusing on gender as its central category of analysis.

HELENE SILVERBERG.

Princeton University

Sandinistas: The Party and the Revolution. By Dennis Gilbert. New York: Basil Blackwell, 1988. 234p. \$24.95.

Dennis Gilbert's book about the Nicaraguan revolution is self-consciously descriptive rather than argumentative: "*Sandinistas* was written for the general reader" (p. ix). No knowledge of Nicaragua or of Marxism is assumed. Hence, the book begins with a historical introduction, a political chronology, and explanations of Marxist concepts (e.g., "Marx was a brilliant political sociologist" [p. 24]). The work is divided into two parts. Part 1 explores the identity and intentions of the Sandinistas; part 2 explores selective facets of the Nicaraguan revolution: agrarian policy, the "bourgeoisie," the Catholic Church, and relations with the United States.

In part 1 Gilbert is at his best. The Sandinista National Liberation Front (FSLN) dominates Nicaragua; and Gilbert chooses well in focusing on it, dissecting it, and analyzing it. He asks—and answers—all the right questions about the party.

The FSLN is presented as a vanguard organization in the Marxist-Leninist tradition of a self-selected, disciplined elite. The party is bent on directing a transformation of society. The concept of a vanguard is distinguished from other elitist conceptions of leadership in its emphasis on mobilizing and transforming, rather than neutralizing, the "masses."

Gilbert details the origin of the FSLN, its membership, leadership, intentions, and strategy. The costly route to power is traced. Most engaging is the discussion of the emergent relationship among the party, the state, and "mass organizations." The influence of the

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FSLN reaches deep into the state. The FSLN's National Directorate (the vanguard of the vanguard) reserves for itself the definition of major lines of political economy, military doctrine, agrarian reform, and foreign policy. And cadres hold key appointments throughout state bureaucracies. The party is linked to the general population through newly established Sandinista mass organizations.

Gilbert's description of the Sandinistas is the result of numerous trips to Nicaragua. There he did not get his hands on confidential party documents that "spilled the beans." Instead, he laboriously collected and sorted the printed speeches and interviews of the ruling Sandinistas. These recorded remarks were augmented by circumspect conversations with low-ranking party members and more free-ranging discussions with bureaucrats and citizens. Residence in the provincial city of Matagalpa offered a chance to see the interplay of party, state, and mass organization firsthand.

Gilbert's book is novel because he openly employs the concepts of Marxism-Leninism, especially the notion of vanguard. Most of the academic writing on revolutionary Nicaragua is by scholars of the "Left" who assiduously shun the lexicon of Marxism-Leninism when discussing the Sandinistas. Their intention is to avoid fueling perceptions in Washington that the regime is totalitarian. Gilbert, who shares his colleagues' political sympathies, departs from custom in his employment of Marxist-Leninist constructs. And the Sandinistas' organization and strategy is convincingly shown to be textbook Leninism. But Gilbert does not leap to the conclusion—nor does his marshaled evidence suggest—that Leninist party organization leads to totalitarianism. He does say the vanguard is literally and inescapably paternalistic (p. 34). Yet it is also well meaning, cognizant of obstacles, and shrewd.

In the latter half of the book Gilbert covers many different issues in the Nicaraguan revolution. Here, he is not as path-breaking or compelling; but he competently explores the fruits of Sandinista initiatives, enabling him to fulfill his aspiration to write an all-encompassing treatment of the Nicaraguan revolution.

FORREST D. COLBURN

Princeton University

Tinker, Tailor, and Textile Worker: Class and Politics in Egypt, 1930-1952. By Ellis Goldberg. Berkeley: University of California Press, 1986. 234p. \$30.00.

The subject of this book is the relationship between work and politics. It examines how factory employment shapes the political activity of an emergent working class, taking the important but relatively unknown case of industrialization in Egypt during the two decades prior to the 1952 military coup. According to the author, most accounts of working-class politics, particularly in the Third World, pay insufficient attention to the concrete forms that conflicts take within the factory. In such conflicts, Goldberg argues, workers will adopt different political strategies and organizational methods according to the different relations of hierarchy and discipline they experience at work, and the different resources provided by the factory's particular social, economic, and political environment. The forms these conflicts take and their success or failure are what mold the workers' wider political views and allegiances.

As so often in initial Third World industrialization, the rapid growth in Egyptian industry during the second quarter of the twentieth century was predominantly a growth in textile manufacturing. Besides the dozen or so big textile mills, however, there were three other large industrial employers: a sugar-processing industry with a refinery and several crushing mills, a cigarette manufacturer partially owned by British-American Tobacco, and a Shell oil refinery at Suez. Goldberg shows that in all these industries workers suffered broadly similar hardships and fought their employers over similar grievances—pay, promotion, working conditions, freedom to organize, and job security. Only in textiles—and in fact only among one group of textile mills—did a radical working-class movement take hold.

Why did one group of textile workers organize and support communist-led unions, while workers' organizations in other industries supported the anticolonial but otherwise relatively conservative Muslim Brotherhood or even the party of the nationalist bourgeoisie, the Wafd? Goldberg explores the larger question of how factory work shapes political affiliation through a close examination of this concrete issue. Radicalism cannot

be explained simply as a result of low wages, of former peasants' experiencing the shock of factory regimentation, or of former artisans' resisting the move to mechanization. These elements were of equal or greater significance in other factories.

Goldberg finds the explanation, instead, in the internal organization and social and economic context of textile manufacture. Because of the complexity of the manufacturing process and the skills required, he argues, textile workers had "careers" (in Charles Sabel's terms) that needed the protection of strong unions that spoke the language of workers' rights. Union members in the sugar mills, by contrast, were the supervisors of an unskilled work force and preferred the Muslim Brothers' language of loyalty, industriousness, and the "moral obligations" of the employer. The Muslim Brotherhood, moreover, portrayed injustice toward workers as the result of foreign control of Egyptian industry. The textile industry was largely Egyptian-owned, however, so such religious and nationalist rhetoric made less sense there than more radical, class explanations.

The textile sector was also less stable and profitable than oil, cigarettes, or sugar. The latter enjoyed local monopolies and low labor costs in relation to the value of output, so they were less vulnerable to strike pressure and more able to make concessions when necessary. Among textile manufacturers, Goldberg shows, the numerous smaller mills concentrated in the northern suburbs of Cairo were particularly weak because workers sacked for labor organizing could find jobs in neighboring plants, so that blacklisting and intimidation by employers were less effective there than in the larger mills located in the Nile Delta.

The intimidation and violence repeatedly employed against the industrial work force are a sufficient reminder of the significance of its politics. As Goldberg remarks, however, the extent of this repression means that workers' groups were often organized in secret and left no record of themselves, making it difficult to reconstruct a wholly reliable picture. *Tinker, Tailor, and Textile Worker* is nonetheless an important study of a much-neglected area of comparative political enquiry.

The book's introduction and conclusion frame the study, somewhat simplistically (given the complexity of the work itself) in

terms of workers' choices. A radical movement did not take hold in other industries besides textiles, Goldberg says, "because other strategies worked better [or] made more sense." This may be true, but the book's contribution lies not in proving that Egyptian workers, given their circumstances, made rational choices but in showing us the complexity of those circumstances and how they shaped not only the particular choices offered but the entire calculus of workers' lives.

TIMOTHY MITCHELL

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The Politics of Coalition Rule in Colombia. By Jonathan Hartlyn. New York: Cambridge University Press, 1988. 332p. \$42.50.

Democracy in Colombia: Clientelist Politics and Guerrilla Warfare. By Jorge P. Osterling. New Brunswick: Transaction, 1989. 353p. \$39.95.

After the assassination of Senator Luis Carlos Galán in August 1989 and the drug cartels' declaration of total war on the Colombian state, the news media elevated Colombia to the forefront of its coverage. However, pervasive violence in Colombian society is limited neither to the contemporary period nor to the drug mafias. The magnitude of violence, along with waning support for the political system by citizens not espousing violent solutions, threatens the very survival of the Colombian state. To what extent does the political crisis reflect traditional political structures' rigidity and failure to adapt either to a rapidly changing society or to the elimination of the norms governing interparty competition during the National Front? What are the prospects for the emergence of a more stable democratic regime in Colombia? These questions orient both *The Politics of Coalition Rule* and *Democracy in Colombia*.

Each book is based on extensive field research. Aggregate data on the Colombia economy, electoral returns, violence, and social change are plentiful in both studies. While each author draws heavily upon indigenous scholarship, Hartlyn shows a mastery of the comparative politics literature as well. None-

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theless, the two books are quite distinct in terms of the audience to which they are directed, their theoretical richness, their professionalism, and the authors' comprehension of Colombian politics.

Democracy in Colombia "grew out of the recognition of a gap in recent, comprehensive writing in English on contemporary Colombia" (p. xii), which it is designed to fill, particularly for those who are unfamiliar with Colombian politics. The study offers a synopsis of Colombian political history, governmental and societal structures, interest groups, and violence. It is highly descriptive, legalistic, and unclear about relationships between the principal actors and how they relate to the political system. Its strongest contributions are the overview of political and governmental structures along with the groups engaged in violence.

The Politics of Coalition Rule is a marked contrast to Osterling's book. It is theoretically sophisticated, extremely well researched, lucidly written, and demonstrates the author's firm grasp of the intricacies of Colombian politics. Hartlyn's principal thesis is that the task of consolidating a stable, legitimate political system has been complicated by the transition from a limited democratic regime (based on restricted competition between the two traditional parties as dictated by the "rules of the game" during the National Front) to a political system that is more open and participatory. The author adopts a "regime-centered" analysis, focusing on the role of political parties, state institutions, interest associations, policy processes, and electoral dynamics. He argues that the National Front was necessary to end acute interparty conflict but that by the late seventies political realities that led to the establishment of consociationalism had changed, thereby dictating transformations in the regime (p. 196).

Both studies underscore the rigidity of traditional parties as a major reason for the current crisis, especially their continued reliance on clientelistic networks for electoral support and their reluctance to revamp party structures. Hartlyn conceptualizes Colombian political parties during the early years of the National Front as functional alternatives to segments characteristic of consociational theory. But as he admits, parties can no longer be viewed in this manner because of waning partisan attach-

ments. Although the traditional parties certainly have contributed to the current crisis, both studies, especially Osterling's, distort the difficulties of regime transformation by stressing the negative role of parties in reformulating the rules of the democratic game. Colombian politicians have not uniformly opposed political change; and the parties have adapted somewhat to changing realities, as reflected in the political and administrative reforms at the local level, the creation of the Unión Patriótica, selected mobilization of new groups, internal party reforms, and the adoption of President Barco's government opposition scheme. Moreover, clientelistic practices and political patronage increasingly fail to predict electoral outcomes, especially in presidential races.

Although the consociational framework developed in *The Politics of Coalition Rule* is far superior to the atheoretical approach of *Democracy in Colombia*, it is not without problems. Its theoretical utility and relevance for explaining Colombian or Latin American politics is limited. Consociational theory represents an easy post hoc explanation of elite political behavior, yet its satisfactory operationalization is complicated. In the Colombian case, a long tradition of interparty coalitions prior to the National Front and political change associated with the modernization process make it difficult to disentangle the impact of consociational rules from other determinants of political behavior. However, consociational theory does provide a coherent framework for Hartlyn's analysis, even though his principal conclusions do not necessarily flow from consociationalism.

Readers unfamiliar with Colombian politics may find *Democracy in Colombia* helpful, despite factual errors and dubious conclusions. (The 1951 elections were not contested [p. 160], the 1968 constitutional reforms did not result from ANAPO pressure [p. 101], the "Alvarista" faction is hardly ultrarightist [p. 182], and Catholic universities may well not have higher academic standards [p. 255].) *The Politics of Coalition Rule* is thoroughly professional and destined to become a classic not only for its superb analysis of Colombian politics but also for its contributions to the study of comparative politics, particularly with respect to the limits of consociational rule in changing societies. Both studies underscore the

formidable constraints associated with the emergence of a stable democratic regime in Colombia.

GARY HOSKIN

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The Boulanger Affair Reconsidered: Royalism, Boulangism, and the Origins of the Radical Right in France. By William D. Irvine. New York: Oxford University Press, 1988. 239p. \$34.95.

In 1886, 48-year-old General Georges Boulanger became minister of war in one of the many cabinets of the Third French Republic. His militant republicanism, his reforms, and an anti-German orientation appealing to *revanchard* sentiment produced a high public profile and wild popularity. In 1888, out of office, Boulanger cashed in his popular standing through a sweeping election campaign whose repeated successes made him appear a threat to the republic. His plebiscitary campaign and demagogic style elicited support on the radical republican Left and funds from the royalist Right, both intent on upsetting the insecure regime. But Boulanger's meteoric career ended as abruptly as it began. In 1889, in the wake of winning a hard-fought election in Paris itself, he let himself be intimidated by vague government threats of prosecution and sought refuge abroad; Boulangism fizzled out. Two years later, its cashiered and discredited leader committed suicide in Brussels on the tomb of his mistress.

The story of Boulanger's rise and fall has attracted numerous historians, both U.S. and French—the more so because it marks the beginnings of modern mass politics based on the conjunction of nationalism and socialism and also the end of conservative royalism in French politics. William Irvine, who teaches history at York University, takes up the familiar tale in order to set some of its aspects straight, notably the quality of radical commitment among Boulangists, the weight of royalist participation, and the effect of the Boulangist adventure on the fortunes of the Royalist party.

In sum, Irvine argues, most Boulangist candidates were not so much radical as oppor-

tunistic, like the general himself; and of those who had principles many—perhaps most—would eventually gravitate toward the conservative or reactionary Right. Nor can alliance with the royalists be dismissed as a temporary leftist expedient. It was crucial to Boulangist success because conservatives provided essential funds but also because, without royalist votes, neither Boulanger nor his republican followers would have achieved electoral success. As for royalism, condemned by inherent anachronism and inept leadership, the Boulangist alliance was its last chance. Unable to adapt to new political conditions, royalism foundered after Boulanger's failure, as it had been on the verge of doing before he appeared.

The argument that Boulangism was a flash in the royalist pan is clearly made and largely convincing. The narrative is solidly grounded in archival material, notably the papers of the royal pretenders and of their chief advisers. But I am left uneasy by the consistent equation of royalism and conservatism. They frequently coincided, of course. But if the royalists of the 1880s were certainly conservative, their conservative contemporaries were by no means all royalist. So when Irvine concludes that the enduring legacy of Boulangism would be a new style of conservative politics, I am moved to restrict that phrasing to the marginal Right, where the heritage of Boulangism is more obviously situated.

Finally, like most chroniclers of the brief encounter between an ambitious soldier and plebiscitary politics, Irvine underestimates the record of Boulanger before Boulangism. The man whom he dismisses (as contemporary critics did) as having no significant military record and doing little of lasting significance as war minister, had a brilliant career as a fighting officer and was wounded and decorated for bravery several times: in Italy, in Algeria, in Cochin China, during the German siege of Paris, and in street fighting against the Communards. As minister of war, he improved living conditions for rankers (he had already reformed NCO training while director of infantry); founded the Cercle Militaire for officers; helped to introduce the Lebel rifle, the use of smokeless gunpowder, the explosive Melinite, and even the newfangled telephone, which he had to force upon his subordinates.

Boulanger was an operator and an oppor-

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tunist, as every general has to be, though obviously not slick enough in either role to get as far as he had hoped. Dropping one's political catches because one is not ruthless, lawless, or decisive enough can be fatal. It was for Boulanger, but it leaves us with a personality more complex and more interesting than the author admits.

EUGENE WEBER

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Feminization of the Labor Force: Paradoxes and Promises. Edited by Jane Jenson, Elisabeth Hagen, and Ceallaigh Reddy. New York: Oxford University Press. 1988. 295p. \$39.95 cloth, \$14.95 paper.

This excellent set of essays by Canadian, U.S., and European scholars analyzes the impact of the dramatic increase of women in the labor force of Europe and North America since World War II. Among the topics considered are the promise and reality of women's opportunities and progress, the growth of part-time employment, the challenge of new technologies, and the effect of the double burden of work and family obligations. As the title suggests, the authors also explore prospects for feminization of the labor force, a phrase used to signify not only the increase of women in, but also the humanization of, the labor force. The anthology describes the basic environmental and working conditions since World War II in seven major Organization for Economic Cooperation and Development nations, followed by an essay with labor force data for each nation. After the introduction of key issues in the early chapters, there are three major sections examining the changing labor force, social policies, and women's identity.

A number of recent books have analyzed the changing roles of women in comparative perspective. Some have focused on the workplace, others on the political process, and still others on societal climate and conditions. To some extent, this book tries to combine all of these concerns. The issues discussed are far-reaching, including state policies, family issues, social attitudes, and economic modernization. Each country is singled out in one or more case studies. For some countries the focus is primarily on women in the labor force. For

others the focus is on women and social policy. Although not mutually exclusive, the different emphases result in less-than-parallel treatment of each nation. Few countries are discussed in more than two essays. The evolution of social policy in the United States receives considerable attention, whereas there is less discussion of women in the U.S. labor force.

The introduction states that the presence of women in the labor force could be a humanizing process, if the political actors, including women, are willing to allow the workplace to become humanized. This theme is not consistently pursued in the ensuing chapters. The editors conclude that the differences among the countries of Europe and North America are so great that one cannot make generalizations about women's work, the workplace, or workers (p. 14). I do not entirely agree with that conclusion. Careful analysis of the data and commentary suggests that although women's position in the workplace has improved in recent decades, there are still many barriers to overcome. Even in Sweden, often the barometer by which other states measure their progress, Ruggie describes occupation segregation, less-than-equal pay and some psychological problems besetting women workers. In other countries, the unfinished agenda is more extensive.

Among the dominant themes in the various essays is that women are quite often employed in the part-time, nonpermanent work force or in the unofficial economy, outside of the mainstream of full-time workers with benefits. Italy, one of the few countries in which women are not heavily in the part-time ranks, has laws mandating that part-time workers must receive equal benefits with full-time workers, a factor inhibiting the growth of part-time labor. On the other hand, the unofficial economy in Italy is large. An important theme in a number of the countries is that women are not considered part of the core work force, which Jenson, one of the authors, termed the "and the" phenomenon. An important similarity among the countries is that the rapid growth in the number of women employees is often a corollary of the growth of the service sector. Italy and Germany, two countries with lower percentages of women in the work force, also have a smaller service sector. Germany has a much higher percentage of workers in manufacturing than does either Japan or the United States, while

Italy handles some social services through alternative means.

In several of the more theoretical, interpretive essays in particular, one finds both explicitly and implicitly a tendency to refer to Marxist and neo-Marxist scholarship. Certainly, some Marxist economic terminology can be useful; but is the Marxist economic model relevant to the modern postindustrial society? Marx studied some of the most developed societies of his time, but they were primitive versions of capitalism. Most of the postindustrial societies studied have mixed economies or capitalist economies infused with a strong dosage of the welfare state, hardly the unbridled capitalism of Marx.

Each essay appears to be well researched and referenced and has an excellent bibliography to facilitate further study and research for the scholar and student. Written from an interdisciplinary perspective, the work can be used by the economist, sociologist, and historian as well as the political scientist.

A weakness of the anthology is the failure of the editors to tie the disparate essays together for the reader. The editors assume that the introduction will serve both an introductory and summary function, but I think that the work would have been strengthened by a concluding, summary chapter.

NORMA C. NOONAN

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Thanks to God and the Revolution: Popular Religion and Class Consciousness in the New Nicaragua. By Roger N. Lancaster. New York: Columbia University Press, 1988. 244p. \$37.50.

With the Sandinista revolution in Nicaragua having celebrated its tenth anniversary, numerous academic and journalistic publications have focused on this most recent revolutionary transformation in Latin America. Roger N. Lancaster's *Thanks to God and the Revolution: Popular Religion and Class Consciousness in the New Nicaragua* stands out as an eloquent and cogent analysis. It focuses on a theme that is central to the understanding of postrevolutionary Nicaragua—the role of religion in the development of a popular revolutionary culture. Lancaster, challenging a main

tradition in Marxist thought, argues that “not only is religion not inimical to revolution . . . but revolution, in fact requires religion—both rational and irrational—in a manner that is inherent in class solidarity, revolutionary mobilization, and social reconstruction” (p. 21).

Seeking to establish the significance of religion in the Sandinista revolutionary project, Lancaster examines the roots of liberation theology as it manifests itself in the emerging social formation. He argues that the evolution of a revolutionary church allied with the Sandinistas is based on the foundations laid by the “popular religion” of a prerevolutionary society. For example, Lancaster holds that the practice of “leveling” (which ensures stable class relations in traditional society) central to the tradition of popular religion, anticipates “socialism’s appeal to peasants and the recently proletarianized poor of Latin America” (p. 54). Thus, the metaphors of revolt alive in the rituals of popular religion materialize in the insurrection of the masses against the Somoza dictatorship.

The author proceeds to examine the links between liberation theology and revolutionary ideology. He interprets liberation theology as the glue that keeps the fabric of the revolutionary society together. Whereas it is certain that this “religion of hope” (p. 55) embodies the belief of the Nicaraguan masses in the revolutionary project and has found its way into Sandinista discourse, one could question Lancaster’s proposition that the three main ideological strands evident in the Sandinista project—Sandinism, Marxism, and Leninism—have been “relegated to dependent status: parasitic systems living in the body of the revolutionary Christ” (p. 57). This powerful metaphor negates the central role of Sandinista ideology as the catalyst of the successful insurrection. Liberation theology is without doubt a crucial ingredient in the success of the Nicaraguan revolution; nevertheless, it is not the sole contender to claim the status as the hegemonic force behind the Sandinista project.

The chapter on evangelical Protestantism is of particular interest and provides much-needed information on the rise of fundamentalist Christian sects in post-revolutionary Nicaragua. Lancaster argues that “evangelical Protestantism has been as vigorous a movement among the poor as liberation theology” (p. 104). He establishes the communalities of

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these two manifestations of popular belief, emphasizes the strength of religious activism that characterizes the Protestant sects and notes that the marginalized sectors of society are most likely to embrace the fundamentalist message. The author discounts the predominant view that the evangelical movement in Nicaragua poses a counterrevolutionary threat. He points out that the proliferation of evangelical Protestantism has been encouraged by Sandinista authorities, seeking to weaken the "moral and spiritual authority of the conservative Catholic clergy" (p. 116).

Lancaster also analyzes the foundations of Sandinista legitimacy, focusing on the "indistinguishability and synchronicity . . . of religion and politics, tradition and revolution" (p. 129). He examines the mythology of the Nicaraguan revolution and suggests that the "Sandinista authority derives from the same fount as priestly or saintly authority" (p. 139). The "charisma of the guerrilla" (p. 132) sacrificing him- or herself for the masses in the image of Christ awards legitimacy to the Sandinista project. These "heroes and martyrs of the revolution" constitute the fuel for the revolutionary project. Thus, Lancaster argues convincingly that the legitimacy of the Sandinista revolution does not rest with the economic performance of the government but is derived from the powerful mythology that Sandinismo acquired in the eyes of the people during the years of struggle against the dictatorship.

The experience of the Nicaraguan revolution leads Lancaster to revise Marx's view on religion as expressed in the early writings. He argues in light of the later work that Marx revised his position that religion necessarily signifies alienation. Rather, Marx's condemnation of religion should be understood within the historical context of the early writings. The Nicaraguan brand of socialism incorporates and emphasizes liberation theology in contrast to the official doctrine of other revolutionary regimes and constitutes significant evidence to support Lancaster's view that Sandinista Nicaragua makes a major contribution to the realignment of Marxist-Christian relations.

ILJA A. LUCIAK

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Elites and Political Power in the USSR. Edited by David Lane. Hampshire, England: Edward Elgar, 1988. 299p. \$42.95 cloth.

Elites and Political Power in the USSR explores the Soviet leadership, apparatus personnel, and the role of the elite as they have changed with the rise to power of Mikhail Gorbachev. The volume, which grew out of a 1987 conference at the University of Birmingham, represents the best of what the editor labels a "positional" approach to Soviet elite politics. As such, the contributors concentrate more on the impact of the individual background characteristics, patronage chains, and institutional affiliations of the elite and less on the actual "decisional" or policy-making aspects of elite politics. The adoption of this analytic point of departure highlights the increasingly diverse nature of those elements comprising the governing elite and necessarily discounts the relevance for the Soviet setting of such concepts as "ruling class" and "power elite," both of which imply a level of elite consensus and cohesion that no longer exists.

While the contributors to the volume represent a rather diverse group in themselves, they are in accord on the central philosophical approach to the issue of elite politics in the Soviet Union. That approach has been variously labeled in the literature: "institutional pluralism," "participatory bureaucracy," "bureaucratic pluralism," or "pluralism of elites." Each chapter explicates a different set of interests and actors, a different list of constraints on executive action, or a different account of the limits of central power without exaggerating the impact of these forces on the operation of the Soviet system. Combined, the contributors to this volume go a long way toward providing the definitive statement for the adoption of the institutional-pluralist model of Soviet politics.

In essence, this approach postulates that as the Soviet system enters its postrevolutionary or postmobilization phase, more rational-technical aspects of governance become increasingly important for the maintenance of power and authority. As Soviet society develops, responsible cadres are increasingly evaluated on technical or performance criteria; over time, ideological concerns are less significant in elite recruitment and mobility. As Reisinger and Willerton put it, "The old distinction between Party hack ideologues and trained experts is

not relevant to this post-mobilization Soviet reality" (p. 101). In addition, diverse interests develop with the elite and the locus of political activity in the Soviet system is seen to lie within and among groups. The contributors to this volume present the picture that these groups are (1) institutionally based, (2) relatively autonomous, (3) only weakly organized as pressure or demand groups per se, and (4) both internally and externally conflictual. The dynamic of group interaction over alternative policy and administrative options is, then, the dynamic of the political system. In this atmosphere, whose development is being accelerated under Gorbachev, a fresh look at the forces affecting the Soviet elite is needed.

The book is divided substantively into four sections that focus, respectively, on the issues of (1) authority building, (2) elite mobility and stratification, (3) institutional interests, and (4) center-periphery relations. The research efforts that comprise these sections are the beneficiaries of two fine theoretical pieces that succeed in providing the contextual parameters of the volume. Lane deserves credit for overcoming many of the weaknesses of edited conference volumes. The presentation and focus remain—with perhaps one exception—tight throughout; the individual contributions make repeated and pertinent references to other chapters and other positions taken in the book.

While the book aims to present substantive findings relevant to the new developments in Soviet elite politics, several of the chapters also provide insights into the methodological dictates of elite analysis. Especially noteworthy in this regard is the innovative contribution of Michael Urban. The book evenly presents both quantitatively oriented and more traditional analyses and examines both national and subnational trends. The 15 contributors are among the leading Western analysts of the Soviet elite and together present to the reader the latest research trends in U.S. and European scholarship on these topics. In sum, *Elites and Political Power in the USSR* is a highly valuable and timely contribution to the extant literature on Soviet elite studies and should command the attention of upper-level students and specialists.

WILLIAM A. CLARK

University of South Carolina

Unions in Politics: Britain, Germany, and the United States in the Nineteenth and Early Twentieth Centuries. By Gary Marks. Princeton: Princeton University Press, 1989. 256p. \$35.00 cloth, \$13.50 paper.

Fair Shares: Unions, Pay, and Politics in Sweden and West Germany. By Peter Swenson. Ithaca: Cornell University Press, 1989. 272p. \$29.95.

Both of these books are exceptionally sound examples of recent scholarship on trade unions and politics in advanced industrialized countries. Yet despite these similarities, the two books take very different perspectives in the level of analyses that they use to study the politics of trade unions. Marks's approach stresses the diversity of trade unions within particular countries. He argues that only by understanding why unions diverge can one begin to search for the factors that might be able to forge a new kind of unity. Swenson, on the other hand, takes as his unit of analysis the more traditional approach of understanding union action by looking at peak national associations and lamenting how the diversity among some of the constituent unions impedes an effective national level set of policies for trade unions. In other words, for Swenson, too much union diversity impedes the collective solidarities that national trade union movements are supposed to be all about. Thus, in their units of analysis—and their implications for what makes effective trade union movements in both the market and political arenas—the two books take very different paths. I maintain, however, that these paths must be brought closer to one another if scholars of the politics of trade unions wish to maintain that organized labor remains a strong, progressive force in capitalist societies.

Although Marks's work concentrates on trade unions in Britain, Germany, and the United States at the turn of the century, it is particularly relevant in illuminating the issues that face unions as they approach 2000. Marks's focus is on subnational levels of organized labor and represents a "second generation" of researchers on the politics of trade unions (Anthony Daley, Miriam Golden, Carol Mershon, and Kathleen Thelen are, with Marks, the most significant) that has gone below the study of peak national trade union confederations to look at unions in specific in-

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dustries and regions. In these three countries, Marks notes that the oft-categorized general types of national-level union response in these countries—"business" unionism in the United States, "labor" unionism in Britain, and "socialist" unionism in Germany—are less convincing in explaining the variations within different industries in the three countries.

In looking at printers' unions and coal miners' unions in the three countries, Marks finds substantial patterns of variation within countries but striking patterns of convergence within the coal-mining and printing industries across the three countries. The evidence offered is unions' responses to repression; their preferred forms of political action; and their encounters with, and responses to, the organizational revolution that shaped the unions' choice of craft or industrial unions. For Marks, the diversity of conditions represented both an opportunity for unions to forge collective solidarities based on the specific conditions that affected the workplaces and the communities within which these unions existed. Such sensitivity enabled these unions to determine when to choose the market arena and when to choose the political arena in pressing their demands.

Community-based concerns might seem to introduce a particularistic diversity that would undermine collective union solidarities, a dominant theme of Swenson's book. Marks implicitly acknowledges this theme in the context of a new look at the "American exceptionalism" debate. For Marks the question, Why no labor party?—not, Why no socialism?—should be more relevant for the United States. In this context, the kinds of unions (craft-oriented), union decision making (against the formation of an independent labor party) shaped by national political contexts, and the role of socialists toward the labor party question (antagonistic) were powerful factors that impeded the formation of a mass-based U.S. labor party. Thus, missing from the U.S. labor movement was the diffusion of a wider sense of "community" that approached the spirit of "universalism" so important for Swenson's collective solidarities.

The primary lesson to be learned from *Unions in Politics* is that effective collective solidarities must first be forged in the specificity of union responses to the needs and concerns of their members in particular sectors

and regions. But precisely because a primary line of demarcation for organized labor—then as now—is the diversity among unions in one country, it makes the macro issue of inter-union universalism (Swenson's primary concern) harder to attain.

Swenson, on the other hand, basing his analysis on the West German and Swedish labor movements, sees independent action on the part of industry-specific trade unions as a profound threat to collective solidarities that peak-level national union confederations have fought long and hard to secure. *Fair Shares* is essentially about union centralization and political power in Sweden and West Germany and how only centralized union movements can structure market outcomes in the "political economy" and ensure a fair distribution of wages in the "moral economy." But in using universalistic appeals to appeal to unions facing increased regional and sectoral fragmentation, a "wage policy trilemma" is created that makes unions have to settle for, at best, only two of the three components of the trilemma: full employment, higher wages, and equitable wage differentials.

Looking at nineteenth- and twentieth-century evidence, Swenson argues that centralization gave unions the ability to standardize conditions and prevent employers from escaping from arrangements that unions had forced upon employee peak associations in a defensive response to union centralization. It also gave national peak union confederations strength against their constituent members. In this context, the solidaristic wage policy of the Swedish Labor Confederation proved a major moral force, as it convinced higher-paid unions that bringing up wages of lower-paid ones was crucial for collective goods for all union members. As Swenson argues, "Nation-wide struggle with employers leading to national contracts probably added to the moral force of distributive norms that union officials used against internal opponents" (p. 39). Also, in Sweden, the importance of exports and the dynamics of parliamentary politics helped employers and unions move to a more centralized system. In postwar West Germany, the less-centralized trade union movement (which allowed the largest union, the metalworkers in a spirit of trade union "autonomy" to set the leading pace on wage negotiations rather than a more universal national-level pattern) pro-

duced a slightly weaker union position in both the "moral" and "political" economies.

However, in looking at the partial erosion of interunion solidarities in both countries due to the regional sectoral disparities of the 1970s and 1980s, Swenson sees that the Swedish and German attempts to turn the wage earner funds as a defensive, rather than offensive, union response. Yet because Swenson emphasizes the macro level manifestations (and distributional rather than productionist aspects) of the wage earner funds, he misses the opportunity to use micro level worker concerns (compare Marks's sense of community and control of the workplace) as a foundation to mobilize rank-and-file union members for progressive macro level goals. Thus, he fails to outline a union vision of the investment issue at a time when worker dislocation, capital mobility, and international financial flows suggest the need for a more flexible yet still universal union movement. Trade union researchers in the 1990s must try to reconcile the issue of unity and diversity by synthesizing and linking micro and macro level union concerns.

CHRISTOPHER S. ALLEN

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Bureaucrats and People: Grassroots Participation in Third World Development. By John D. Montgomery. Baltimore: Johns Hopkins University Press, 1988. 140p. \$22.50.

Over the last three decades much has been written and said about the role of participation in the design and implementation of development programs and projects. For this literature, Montgomery's succinct and straightforward book constitutes a refreshing reexamination of the links that must bind citizen and bureaucrat together if durable results are to be achieved. This particular collection of essays also offers the reader a useful summary of the writing and consulting that the author has pursued over the years in the search for development activities that would both improve conditions of life for the poor majority in developing nations and enhance democratic institutions despite the obstacles.

The concepts warranting particular attention in this book are those of bureaucratic

populism ("the process by which bureaucrats try to help people participate in managing public policies" [p. 3]) and the identification of critical incidents (which for him are the most effective ways to specify the day-to-day concerns of public and private sector managers in developing countries [pp. 59ff.]). Throughout the book these concepts are developed as much in theory as in practice through frequent example, discussion of specific applications, and summary statements of the lessons learned or opportunities missed in development work.

For those concerned with the design of development programs that are compatible with building and sustaining democratic institutions, a major premise in the literature has long been the view that participation helps development. Not only did this value receive endorsement in the technical assistance legislation of the late 1960s and undergird multiple studies and reports on the administration of development activities during that decade, but it also underlies most of the rural development literature of the 1970s. Likewise, despite the challenge of a newer literature advocating the use of markets, economic incentives, and self-interest to raise performance and increase outputs at the grass roots level, during the 1980s those working in rural development programs have for the most part continued to tie the sustainability of development initiatives to the issue of who participates.

Montgomery's career has spanned this range of experience, and here he endeavors to bring into focus his own particular contribution to this field. Montgomery reviews his earlier empirical work on participation as a crucial factor in the success of land reform programs, discusses why it makes a difference to continue to encourage government bureaucrats to build links with local organizations, and sums up the initiatives in managing irrigation projects and administering land and water resources that have enhanced integrated rural development through decentralization. Beneath the surface is a healthy skepticism and a discussion of why the tension between experts and bureaucrats in these domains remains unresolved when it comes to producing cumulative, long-term results. What we have learned about development, especially in the newer work on private volunteer organizations reflected in his later chapters, has not necessarily led to more informed bilateral assistance, especially when

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one examines the U.S. case. While he does not belabor the point, he elucidates how the passion to control within our own society (despite a rhetoric of contracting work out to specialists) has resulted in an incapacity for institutional learning in linking development work's success at the level of specific projects and programs to broader questions of policy and management.

LAWRENCE S. GRAHAM

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Cocaine: White Gold Rush in Peru. By Edmundo Morales. Tucson: University of Arizona Press, 1989. 228p. \$24.95.

This book is about the social, political and economic transformation of a people in response to the U.S. demand for cocaine. The growing of coca and the processing of its derivative alkaloids has become a major Peruvian growth industry. The ramifications of this activity have not only settled in the backwater regions of the eastern Amazonic uplands where coca is grown but have reached into the heart of Peru's political, law enforcement, and judicial systems.

The effect of this new commerce has been corrosive of traditional society and modern institutions, has placed the country in even more onerous conditions of international dependency, and has solidified new kinds of social class exploitation. The cocaine industry, largely vertically integrated through intimidation and terror by international narcotics cartels centered mostly in Colombia, is likely to create even more socially and politically impoverishing conditions for Peru. The deleterious effects will unlikely be satisfactorily addressed by any current policy applications (e.g., United States-supported crop eradication and crop substitution programs, drug interdiction and other law enforcement efforts, U.S. aid, currently advocated alternative forms of economic development, and so forth).

Morales's work is a social and political ethnography, developed in large part by the participant-observer methods of social anthropology. The author was raised as a Quechua-speaking Peruvian native. He returned to his highland ancestral home with clearly obvious skills that facilitated his movement in and

through the underworld of some of the country's regional cocaine society and economy. The written product is a work that is both insightful and interesting. While largely the anecdotal account of a field observer, with many first-person illustrations, the work also enjoys a respectable foundation in the relevant literature.

Because it is an ethnography, the book is wide-ranging in its subject matter, from discussions about the coca leaf in Andean traditional life to coca culture to the cocaine economy to the politics of control and eradication. Political scientists are most likely to be drawn to the discussions on the political economy of cocaine, political and judicial corruption, and in general the demand, supply, and command structures of an underground economy.

There are several drawbacks with this study, some of which pertain to the research methods, some to the difficulty of researching an underground economy in which the researcher's personal safety is easily placed on the line, and some to the lack of a strong theoretical base. Also, there is some redundancy in the chapters; and Morales does have a tendency to editorialize in an unsystematic way.

On the whole, however, this is an engaging book, a must reading for anyone interested in Peru or in the international political economy of cocaine.

LAMOND TULLIS

Brigham Young University

Transfer of Arms, Leverage, and Peace in the Middle East. By Nitza Nachmias. New York: Greenwood, 1988. 196p. \$39.95.

Living by the Sword: America and Israel in the Middle East. By Stephen Green. Brattleboro, VT: Amana Books, 1988. 279p. \$19.95.

Political scientists have been very slow to recognize that arms transfers often represent a critical mechanism for testing hypotheses about the degree of influence a great power can exert on a small-power client state through the manipulation of military aid as a policy instrument. This is the question addressed by both books, looking at the same patron-client state relationship—the United States and Israel—albeit from very different perspectives.

The central conclusion to be drawn from both books is that the political leverage a major power can exert over a small state, even when the latter depends heavily on military aid from its patron, is much less than might be expected. Standard "explanations" centering on U.S. domestic politics and the excessive influence of the Israeli lobby in Congress are at best incomplete and at worst misleading. There is no analogous "Syrian lobby" in Moscow; and Nachmias notes, quite correctly, that the Soviets have often been no more successful at exerting leverage on Syria (and Egypt before 1972) than the United States has with Israel. International and regional political cross-currents along with conflicting policy imperatives of the major arms-supplying powers combine to blunt the effectiveness of arms transfers as a coercive policy instrument. Both authors reach this conclusion using as a case study U.S.-Israeli relations with respect to U.S. foreign policy goals in the Middle East. In the specific case of U.S.-Israeli relations, Nachmias and Green suggest that the surprising lack of U.S. influence over Israeli policy by transferring or withholding arms has been compounded by U.S. policy myopia combined with poorly conceived initiatives and a misreading of events in the Middle East.

Apart from that general conclusion, there is very little overlap between the two books. Nachmias concludes that using arms transfers as a policy instrument to coerce Israel to accede to U.S. policy goals in the Middle East, particularly a final peace agreement, should not be attempted (p. 184). The prescription that emerges from Green's book is just the opposite: unless the U.S. exerts whatever leverage it might have on Israel, not only will a final settlement of the Arab-Israeli conflict continue to elude U.S. policy makers, but there will be another war.

The two books are not only different in style and approach to subject matter but in underlying themes as well. Nachmias develops a concept of leverage that she distinguishes from the more general concept of power (p. 9-10). She applies this concept to a series of U.S. policy initiatives aimed at securing a final peace settlement between Israel and the Arab states: the Rogers peace plan of 1969, the United States-sponsored Sinai disengagement agreements between Egypt and Israel in 1974-75, the Camp David agreement, Israel's

invasion of Lebanon and the aftermath, and subsequent Reagan administration peace initiatives.

The chapters in Nachmias' book underscore a fundamental point that has been overlooked for far too long. A great power is seriously constrained in the use of arms transfers as a coercive policy instrument if the client state's security is important to realizing the patron power's own goals in the region. If the client state's security is critical to the supplying power's own interests, the great power cannot withhold military assistance to the point where the client's security is truly undermined.

A different issue is whether the great-power patron is obliged to defer to the client's definition of what is necessary to maintain its security against regional threats. Nachmias implies that the United States should defer to Israel's perceptions of its security requirements. She goes so far as to blame Israel's invasion of Lebanon on the United States since Reagan "failed to reduce tensions in the region *before* Israel invaded Lebanon" (p. 156, emphasis original). Even Prime Minister Menahem Begin and his defense minister, Ariel Sharon, who presided over the invasion of Lebanon, did not have the chutzpah to make that claim! This is after Nachmias argues persuasively (e.g., pp. 126-28, 155-56) that the United States simply did not have the leverage to change the political stalemates in the region.

By contrast, the main thrust in Green's book is that the United States has all too often deferred to Israel's definition of its security requirements, always to the detriment of Washington's own policy goals in the region. Green's chapters consist of a series of incidents in U.S.-Israeli relations that show how with the introduction of the F-4E (Phantom) into the Israeli Air Force in 1969, Israel's security goals diverged increasingly with the overriding U.S. objective of achieving a lasting peace in the Middle East.

Green concludes that after 40 years of trying to obtain a safe homeland for Jews Israel has achieved overwhelming regional military superiority, including undisputed control of the air and a nuclear monopoly, but is still not secure. It has won—or at least not lost—every war it has fought since independence, and still it is not secure (p. 233). The territorial buffers Israel has conquered and populated with the help of U.S. military and economic aid and

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political support have, if anything, in recent years made Israel less secure with the outbreak of the uprising (Intifada) in the Occupied Territories. Israel enjoys a unique military and security relationship with the world's strongest military power, yet the Intifada puts the lives of every person under Israeli control more at risk than before. In short, no amount of conquered territory or arms will assure a safe future for Israel. Absent an effective application of U.S. leverage on Israel to accept an exchange of territory for peace, the United States risks another Arab-Israeli war, possibly fought with tactical ballistic missiles. This is a replay of what occurred in 1973 when the United States accepted Israel's assessment that the status quo could be guaranteed indefinitely with sufficient U.S. arms supplies to Israel.

Both books are valuable resources for scholars, officials, and students specializing in U.S. foreign and security policy in general and for people with an interest in the Middle East in particular. The near-total inability of the United States to exert effective influence or to project power effectively in support of its political interests in the region is starkly presented, no matter what the reader's views on what the parameters of a "just settlement" of the Arab-Israeli conflict may be.

LEWIS W. SNIDER

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The Egyptian Bureaucracy. By Monte Palmer, Ali Leila, and el-Sayed Yassin. Syracuse: Syracuse University Press, 1988. 188p. \$22.95.

Given the size of the public sector as well as the extension of the state into many areas of social and economic life in Egypt, it is not surprising that there have been numerous studies of the Egyptian bureaucracy. As the authors point out, there are a variety of ways of studying bureaucracies and bureaucrats. This study, done under the auspices of the prestigious Egyptian research organization, the Al-Ahram Center for Strategic Studies, focuses on behavioral variables to determine whether the Egyptian bureaucracy can manage tasks confronting it into the next century.

The rather pessimistic conclusion is that the Egyptian bureaucracy lacks the capacity to do

its basic job and that much of the reason for this inability lies in the attitudes of the bureaucrats themselves. Following the work of Frederic Herzberg, the authors cast a somewhat skeptical eye on the idea that remuneration (or hygienic factors in Herzberg's language) will be of very much help in improving bureaucratic performance. Pay raises alone will not cause officials to do the tasks at hand, which require energy, flexibility, innovation, and political savvy. The Egyptian bureaucracy is, in the authors' words, "lethargic, inflexible, noninnovative and lacking in rapport with the masses" (p. 151). The somber judgments of the authors that much bureaucratic behavior in Egypt is rooted in elements of Egyptian culture leads them to recommend that in addition to commonly recognized steps, such as raising salaries and wages and tying them to performance, recruitment training, and assignment of the bureaucratic corps at the university level (and before) must be dramatically reorganized.

The study is based on a survey of attitudes of Egyptian bureaucrats at a variety of levels and in three different ministries. The authors make a clear and convincing case that simply raising wages will not engender any automatic increases in the productivity of the bureaucracy. The authors are extremely knowledgeable students of contemporary Egyptian life and have a deep commitment to finding practical solutions to the problems that confront Egypt today. Their book is a welcome addition to the literature on the Egyptian bureaucracy and provides a far more sympathetic and complete picture of the Egyptian bureaucracy than Morroe Berger's *Bureaucracy and Society in Modern Egypt* (1957). The pithy style of this book and the cogency of the arguments presented also make it a welcome addition to studies such as Nazih Ayubi's *Bureaucracy and Politics in Egypt* (1980).

A few methodological points do concern me, however. Because the state controls not only plain vanilla ministries but also production complexes, the authors have included a very wide variety of jobs and roles under the label *bureaucrat*. I think their conclusions might have been strengthened had they focused a bit more on the different tasks undertaken by different kinds of state organizations. They might then have cast their net into some of the literature on labor productivity regarding the public sector, notably Henley and

Ereisha's studies "State Control and the Labor Productivity Crisis" (*Economic Development and Cultural Change* [1987]) and "State Ownership and the Problem of the Work Incentive" (*Work, Employment, and Society* 3 [1989]). I don't believe these studies contravene Palmer, Leila, and Yassin; but they do shed a bit more light on how one might link remuneration to performance in the public sector industrial undertakings.

Secondly, a bit more attention might have been paid to explaining to readers why the authors rely rather heavily on computing gamma to evaluate their data and the meaning of the method. Since gamma measures the predictability of order on one variable from order on another, it simply is not clear to me exactly what the meanings of all the computed gammas are. On some occasions the computation appears to compare scoring by different judges or on different scales. On other occasions, the terseness of the presentation makes it confusing. We are told that "the behavior of officials at the Aluminum Corporation . . . differed from the behavior of officials in the Ministries of Industry and Social Affairs in the following areas: . . . (8) Demographically, they were primarily male ($g = .777$) and were drawn largely from the rural areas ($g = .658$)" (p. 125). I can imagine how gamma might be meaningfully computed here, but it doesn't accord with the sense of the sentence. Because the variables male and rural origin are not themselves ordinarily scaled, the point being made is elusive. I do wish the authors—writing largely for readers in Middle East studies and policy making—had drawn us more fully into their methodological confidence.

This is a welcome addition to our understanding of the Egyptian bureaucracy and the attitudes of the Egyptian bureaucrats. Unlocking the energies now tied up in the bureaucracy will be crucial to Egypt's future, and there is a lot in this book for policy makers to reflect upon.

ELLIS GOLDBERG

University of Washington

Handbook of Political Theory and Policy Science. Edited by Edward Bryan Portis and Michael B. Levy. New York: Greenwood, 1988. 283p. \$45.00.

As its editors are well aware, this is not a true "handbook" if by that term we mean a brief reference work or practical manual. It is, rather, a well-ordered collection of articles with a common concern: the relation between policy science (which is essentially empirical and instrumental, relying very often on economic measures and models of behavior) and political theory (which, in its traditional form, includes moral evaluation of the ends, or goals, of political institutions and political action). This concern has been addressed in such diverse works as Robert E. Goodin's *Political Theory and Public Policy* (1982) and Lance deHaven-Smith's *Philosophical Critiques of Policy Analysis* (1988). Despite the disparity of approaches, these and similar works share a two-fold purpose. First, the authors seek to enrich policy science by increasing its sensitivity to questions regarding justice and the complex social and political milieu in which policy must be made to succeed. Second, they seek to create a legitimate role for moral and political theory in the formulation and analysis of public policy.

Portis and Levy have added to this discourse by organizing the rather idiosyncratic essays included in this volume to represent three general positions on the matter. These positions, respectively, hold the relation between political theory and policy science to be "complimentary," "hierarchically integral," or "mutually exclusive." While the last two categories are somewhat artificial and the book as a whole is, as one might expect, far from conclusive, Portis and Levy do manage to frame the issue in an authoritative way, spelling out the promise, as well as the pitfalls, of combining political theory and policy science.

On the positive side, the essays in the first third of the book, taken together, suggest numerous possibilities for enhancing the expertise that informs public policy. Combining theoretical breadth and depth with the quantitative precision of policy science might well be the ticket for getting us beyond the systemic blind alleys hindering progress in many policy areas. This, in turn, would rescue political theorists and philosophers from the irrele-

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vancy that has helped to define their enterprise at least since Plato. In short, everyone would gain. Policy science would be made more penetrating and therefore more precise in the long run by the vision of political theory. Political theory would become more authoritative. The general public would enjoy wiser, more effective policy.

On the other hand, as the last two sections of the book make clear, the joining of political theory and policy science is problematic. It is not always easy to conceive of the two disciplines as equal partners. If they are not equal in authority, which is to take precedence? Moreover, it may well be that the purposes and methods of policy analysis and political theory are so disparate as to be antithetical. While these difficulties are presented with considerable seriousness, most of the authors do not take them to be insurmountable. They are, rather, meant to be sobering enough to cast the task of synthesis in its full complexity. Also sobering is the essay by David C. Paris at the end of the first section. While Paris finds the two approaches to policy inquiry complementary, he believes that both are unrealistic in their aims. The real world of public policy, according to Paris, allows only for the limited precision and partial vision provided by "rational ideologies" (p. 76) that fall significantly short of the prevailing academic standards in both disciplines.

The proof of the pudding for those who wish to bring about a full partnership between policy science and political theory cannot lie in any theoretical synthesis. What is called for is an actual application so intellectually and rhetorically powerful as to compel broad recognition of the possibilities in this area. This book offers no such application. It does, however, offer clear incentives for attempting such a venture. It also gives a good idea of the obstacles that will have to be overcome for such a venture to succeed.

IRA SMOLENSKY

Monmouth College, Illinois

From Protest to Policy: Beyond the Freeze to Common Security. By Pam Solo. Cambridge, MA: Ballinger, 1988. 214p. \$24.95.

Against the Bomb: The British Peace Movement, 1958-1965. By Richard Taylor. Oxford: Clarendon, 1988. 368p. \$65.00.

Mobilizing for Peace. The Antinuclear Movements in Western Europe. By Thomas Rochon. Princeton, NJ: Princeton University Press, 1988. 232p. \$29.95.

The peace and antinuclear arms movements of the early 1980s are beginning to generate an academic fallout of critical accounts and theoretical analysis. The first and least theoretical of the three books reviewed here, *Mobilizing for Peace* by Pam Solo, is a critical history of the American Freeze Campaign written from the perspective of an activist and political organizer. Richard Taylor's meticulous history of the British peace movement in the 1950s and 1960s organizes movements events into analytical categories to distinguish ideological strands and tendencies. Tom Rochon's study of this decade's peace movements utilizes social and political theory to put the particularities of historical variation into a cross-national perspective.

Solo's book is a useful starting point for explaining the U.S. peace movement of the 1980s. Although the book is organized chronologically, the sequence of events can be restated in more analytical terms. The movement started out with a receptive cultural and political opportunity structure in which the triggering events of increasing hostility between the Soviet Union and the United States, particularly the U.S. military policy in the first half of the Reagan presidency, sparked renewed mass mobilization for disarmament (pp. 27-42). Movement entrepreneurs, many of whom gained their expertise as participants in antiwar movements of the 1960s and 1970s, seized the opportunity; built consensus around a new set of simple, easily understood demands (pp. 42-44); and mobilized resources and allies. Next, the movement spread from the center to the periphery, grass roots involvement increased, and new protest formations were invented, such as local referenda to express popular dissatisfaction with military policy (pp. 94-99). Eventually, the political system responded, particularly Democratic congressional figures. By moving the issue to

the legislative and electoral plane, however, the character of the debate changed. As a consequence, the movement organizers became involved in the maelstrom of Washington politics and were partially coopted into the existing policy networks; while the grass roots movements drifted away, became divided, and demobilized.

In her critical evaluation of this course of events, Solo chastises the willingness of many movement leaders to be coopted into congressional strategies (p. 106) and blames the movement for being unable either to go beyond a simple negative protest to a more constructive policy program (pp. xiv, 15) or to build a permanent organization (p. 19). Solo clearly opposes what must be seen as the dominant tendencies of contemporary movements in the U.S. political system. Compared to other countries, U.S. politics is fairly responsive to protest mobilization and therefore likely to assimilate some of their demands, thus undercutting the evolution of a comprehensive opposition program or of permanent protest organization. Moreover, contemporary social movements express a "fierce individualism" (p. 162) and therefore are unlikely to build lasting, cohesive organizations. If Solo is correct in saying that the peace movement forced Reagan to change his foreign policy course in 1983-84, what is so bad, then, about a partial elite cooptation of the movement? Would Solo prefer a less-responsive, more-rigid system that would certainly invite more encompassing, radical protest movements but with less impact on policy? Although one can argue with Solo's normative and causal presumptions, her book offers a reasonable starting point for an in-depth analysis of U.S. protest in the 1980s.

Richard Taylor's account of the first British postwar peace movement describes a surprising movement trajectory similar to Solo's. Faced with a worsening arms race, a liberal elite instigated a campaign for peace that broadened into a mass movement calling for a total renunciation of nuclear armament. As the movement grew, it became ideologically more diverse, with radicals and moderates veering off in different directions. Again, the response of conventional politics, particularly the Labour party, at first contributed to the rise of the movement and later contributed to its decline when Labour was defeated in the 1959 election and eventually rejected unilateral nuclear dis-

armament at the 1961 party conference. The movement organizations became embroiled in ideological divisions, exhausted their repertoire of protest events. Unlike their U.S. counterparts, however, they became discouraged because they could do nothing to change government policy in an international environment shaped by the Cuban missile crisis.

Richard Taylor carefully describes the leading personalities, ideologies, and practical protest strategies employed by such movement organizations as the Campaign for Nuclear Disarmament (the "moderates"); the Direct Action Committee, later affiliated with the Committee of One Hundred (the "radicals"); the political activists associated with the Labour party; and various radical leftist vanguard organizations. Because Taylor's study excels at analyzing the various organizational problems of the movement (e.g., pp. 153-55, 238-40), the ideological debates (e.g., 181-86), and the problems of strategic choice, it is extremely useful for the theoretical reconstruction of the British peace movement. Like Solo's book, however, it lacks the direct guidance by, and contribution to, theories of social movements and political protest in advanced Western democracies.

Rochon's *Mobilizing for Peace*, by contrast, goes beyond Solo and Taylor to offer the first comprehensive piece of political science scholarship on the European peace movements of the 1980s. It builds explicitly on the theoretical literature and explores the relative validity of "new social movement" and "resource mobilization" theories of protest mobilization (chap. 1). Moreover, it is comparative in nature, drawing on data from the British, Dutch, French, and West German peace movements.

Rochon first analyzes survey data on the sources of dispositions to join the European peace movement. He then compares conventional NATO strategy to various alternative conceptions advanced by peace movement debates. The next several chapters discuss the organization, tactics, and alliances surrounding the European peace movements. One underlying theme running through these detailed, instructive analyses is the tension between the leadership of social movements (intent on gaining maximum impact on defense policy and winning over marginal sympathizers) and many activists (calling for more decentralized organizations and radical forms of

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action). Chapters 7 and 8 explain the response of the political system; first the stances various political parties chose when faced with growing movement mobilization, then the response by the public authorities, such as their ability to keep the protests nonviolent. The concluding chapter pulls together various threads of the analysis and returns to the initial theme of the book, the comparison of theories of movement mobilization.

A thoughtful, lucid book such as Rochon's facilitates critical discussion; but I will confine myself to three points. First, Rochon should make more of the cross-national comparison among peace movements. The selection of countries should be justified in theoretical terms. The study should determine differences and similarities of national peace movements. For example, public opinion data discussed in chapter 2 could have been disaggregated by country. Only much later on in Table 6-1 (p. 147), the reader discovers that the willingness to join peace movements is about five times as great in West Germany as in France. The discussion of the countries, in general, is somewhat lopsided, with the British movements covered in greatest detail and the French movements described very sketchily.

Second, the theoretical argument needs to be reconsidered. Rochon starts out discussing "new social movement" and "resource mobilization" theories as competitors (p. 18) but quickly realizes that they are complementary because each explains different movement attributes (pp. 19-20). Nevertheless, he continues to waver between the two interpretations with sometimes misleading statements, such as that new-social-movement theory views movement as a precursor of revolution (p. 209), whereas resource mobilization theory sees them as moderate. Neither theoretical strand, however, has an a priori argument that movements should be radical or moderate. New-social-movement theorists are concerned only with the qualitative nature of protest demands. According to resource mobilization theory, movement radicalism depends on the political opportunity structure. Rochon also sees limitations to both perspectives but could have elaborated the distinctiveness of his "hybrid perspective" (which claims to be more than a mere amalgamation of both theories [p. 23]) more explicitly in the concluding chapter. Third, although the book is clearly guided by

theoretical concerns, the data in chapters 2-8 should be more tightly connected to the theoretical arguments laid out in the introduction.

Despite these criticisms, Rochon's book clearly represents the major political science contribution to our understanding of the European peace movements of the 1980s to date. It is the most thought-provoking and theoretically incisive of the contributions I have read and is certain to be of major influence on future studies of this subject.

HERBERT KITSCHOLT

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Self-Reliance Versus Power Politics: The American and Indian Experiences in Building Nation-States. By J. Ann Tickner. New York: Columbia University Press, 1987. 282p. \$35.00.

Self-reliance is a development strategy and a normative value that has been articulated by many contemporary leaders in the twentieth century. Ann Tickner's thoughtful book underscores the fact, though, that self-reliance has long meant different things to different people and that the differences are not merely matters of emphasis. Instead, some forms of self-reliance appear to preclude others.

Specifically, a strategy that promotes national self-reliance works at the expense of the self-reliance of individuals and local communities and seems to preclude movement towards regional self-reliance. Tickner's case studies of the early U.S. republic and the first 40 years of independent India indicate that once elites make the decision that national self-reliance is their first priority, any normative preferences for community-based political decision making and localized economic structures fall by the wayside; so, too, do dreams of regional cooperation—to be replaced by hopes of the individual nation's participating with heightened power in the traditional order of nation-states.

Tickner's approach is simultaneously theoretical and comparative and her material ranges widely both historically and geographically; sometimes, however, the links among the theorists of self-reliance and between the theorists and the political actors is not clear. A chapter on Jean-Jacques Rousseau describes his

advocacy of a society built on autonomous subsistence-level agrarian communities and characterized by political equality and decentralized participation; a later chapter describes a similar vision by Mohandas Gandhi nearly two hundred years later. While each of these chapters is interesting in its own right and the analysis fair to both theorists, no real point is made in comparison. One is left with the impression that both are curious fellows who have had no impact. Similarly, the statist position favoring the development of the nation-state is represented by the theorist Friedrich List. While List is often cited by contemporary authors his importance to the national elites with whom Tickner is concerned is obscure.

The most compelling parts of the book, therefore, are the sections analyzing the practical difficulties of attempts to reconcile individual autonomy, national autonomy, and—although this has fallen most quickly by the wayside—regional autonomy. Thomas Jefferson makes for the most interesting case study. As a theorist he favored communitarianism; as a president concerned with U.S. power in a world of conflicting nation-states, he acted on behalf of national power at the clear expense of community autonomy. Similarly Jawaharlal Nehru, although he was never a Gandhian in the sense of accepting Gandhi's economic program or his desire to dissolve centralized political institutions, did, in his early years, favor a more communitarian approach in combination with the development of large-scale industry. With his ascension to the prime ministership Nehru, too, left behind his plans for community autonomy.

Tickner stops just short of arguing that this discarding of communitarianism is inevitable, given the imperatives of the modern nation-state to act in the international system. She rightly makes much of the desires of elites to ensure national security and national influence in the international arena. She makes rather too little, though, of the desire of elites to remain in, rather than eliminate, their positions of power.

Despite the apparent barriers, Tickner retains a fond hope that community self-reliance is possible since in less economically developed countries the statist route has resulted in an increasing gap between rich and poor, the powerful and the powerless. She hopes, too, for a greater success of regional cooperation as

an enhancement to international peace. Most importantly, she sees these two levels of self-reliance—community and regional—as together providing a possible antidote to the power of nation-states. This, of course, would require “radical and widespread structural changes at both the domestic and international levels” (p. 229).

Given her radical conclusions, one wishes Tickner had spent some more time exploring how they might be realized. Her studies of the two democratic states indicate that in neither case has a communitarian philosophy been tried in practice; and in socialist nations, such as China, the strong intervention of the centralized state of course renders doubtful the autonomy of local communities in any meaningful sense. Her preference is for radical changes in property relations and productive techniques, which “would require implementation and protection from political institutions at the national level” (p. 227). How this is to be done while avoiding the pitfalls of a centralized bureaucracy is unclear. She advocates “democratic participatory development” (p. 228) and, unfortunately, leaves it at that.

Still, Tickner raises provocative questions with clarity and style. The book is excellently documented, lucidly written, and addresses a wide range of materials with authority. It can be read profitably by those interested in both the normative and the practical debates surrounding the issue of self-reliance.

LESLIE J. CALMAN

Columbia University

The Catholic Church and Politics in Nicaragua and Costa Rica. By Philip J. Williams. Pittsburgh: University of Pittsburgh Press, 1989. 228p. \$34.95.

This book is a welcome addition to the literature on religion and politics in Latin America. The book carefully analyzes the church's evolution in Nicaragua and Costa Rica, focusing on the last half century. William's book is, I believe, the first detailed account in English of the Costa Rican church, and it provides a balanced treatment of the Nicaraguan church, around which a substantial but often apologetic literature has emerged in the aftermath of the Sandinista Revolution.

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The two countries in question differ in notable ways. Costa Rica boasts the region's longest-standing democracy, while Nicaragua has almost no tradition of democracy and has one of the region's two revolutionary governments. Williams shows that despite these profound differences in political regimes, the Catholic churches of the two countries have evolved in similar ways in the twentieth century. Both churches were conservative and defensive early in the twentieth century, and both eventually opened up somewhat to the reformist impulses stimulated by international church changes. The dominant orientation in the two churches today is still quite similar: the hierarchies are generally cautious, consistently reluctant to promote radical ecclesiastical or political change, and most comfortable with staid liberal democracies in the political order. In Latin America, both hierarchies are cautiously centrist—neither as progressive as the bishops in Brazil, Chile, and Peru nor as conservative as their counterparts in Argentina. Notwithstanding the presence—which has been particularly publicized in the Nicaraguan case—of revolutionary Catholics, Williams correctly underscores that many continuities have marked the histories of these two churches to the present.

While fundamentally emphasizing continuities and similarities between the two churches, Williams also draws attention to some interesting differences. Ecclesiastical evolution in the two countries diverged most in the 1940s when, thanks to the effort of a reformist archbishop, the Costa Rican church began to distance itself from a thoroughly conservative past. The Nicaraguan church was relatively late to follow suit, maintaining its propinquity to the Somoza dictatorship until late in the 1960s. Subsequently, however, the radical sector of the Nicaraguan church developed more rapidly than was the case in Costa Rica.

The author deserves praise for his extensive bibliographic and field work, clear writing, careful and balanced assessments of complex issues, and interesting comparisons between the two countries. Less compelling is his theoretical treatment of religion, of ecclesiastical change, and of some of the not-so-visible complexities of the linkages between the church and politics. First, Williams fails to communicate how important religious motivations are in undergirding political action and institution-

al strategies. After properly criticizing other authors for believing it is possible to separate religious and political actions, Williams essentially does the same by reducing the church to a strategic political institution. This perspective fails to comprehend that the basic conflicts in the Latin American church revolve around different ideas of what religion and church are about. Second, Williams explains ecclesiastical change as a necessary response to maximize church interests. This approach is nonfalsifiable, bordering on the tautological: a phenomenon X and its exact opposite Y are both explained as church efforts to maximize its interests. The problem is that "church interests" are not givens but depend on how one conceives of the church's mission. Conversely, even if a changing environment helps explain why some leaders believed that institutional change was necessary, it does not eliminate the element of choice, agency, and leadership. A societal, religious, and political context may explain why church leaders believe that the institution must undertake changes, but it does not explain why they opt for a particular strategy. The decisive importance of church leadership (and hence, of church choices) is clear in Williams' descriptions of how much the archbishops set the tone of these two churches; but his theoretical framework fails to make use of this insight. Finally, further discussion of some of the contradictions and tensions in the political analysis and action of the various sectors of these churches would be welcome. Rather than focusing almost exclusively on church documents and ecclesiastical discourse, Williams might have done well to look more at behavior and practice.

SCOTT MAINWARING

University of Notre Dame

Washington, Westminster, and Whitehall. By Walter Williams. Cambridge: Cambridge University Press, 1988. 227p. \$25.00.

This book offers a stimulating critique of what the author calls the "pre-modern and anti-modern" character of Britain's institutions of central government. It does a better job of exploring how organizations like the House of Commons and key state ministries might be made more professional than of analyzing the

historical and political roots of the apparently archaic system of British government.

Williams sets out to contrast the efficient virtues of the U.S. policy-making process with the British establishment's cult of the amateur. Central to his argument is the 15-point program of institutional reform he proposes at the outset. This program requires "an unusual giant leap" (p. 186) in the direction of a fundamental reorganization of the state apparatus. At present, central government "is a closed system shrouded in secrecy, poorly managed, and has limited analytic and strategic capability at the top" (p. 1). Members of Parliament have little influence over pending legislation. The lines of authority between minister and permanent secretary are poorly defined. And decision makers don't have access to high-quality policy research employing the latest techniques.

"Key arguments in the book," writes Williams in the somewhat stilted tone he adopts throughout the body of the work, "are that the political/policy balance is skewed towards politics; that more attention to strategy, management and policy analysis are needed in British central government to correct the imbalance; and that these issues are insufficiently understood and discussed" (p. 28). As he argues elsewhere, the "overriding task of the central government must be to build institutional capacity" (pp. 198-99).

Williams proposes that MPs, cabinet ministers, and the prime minister all be served by tough-minded policy analysts chosen from the public and private sectors. Such analysts could be trained at a new college of management, similar to France's *Ecole Nationale d'Administration* or the John F. Kennedy School of Government at Harvard. In order to streamline the policy-making process, parliamentary business could be based on a committee system similar to that developed by Congress in the 1970s. And a U.S.-style freedom-of-information act could ensure that policy prescriptions were disseminated and debated as widely as possible.

In the absence of such a program of reform, he argues, there is the danger that Britain's economic performance will remain uncompetitive with its major rivals and that the famous stability of British life will be disrupted. Reducing government expenditure on public welfare and loosening controls on capital, as the Thatcher government has done, is an inade-

quate response to what amounts to a historic crisis of governmental institutions. But Williams has little to say about the implications of Thatcherism's radical program of economic modernization for the reproduction of existing patterns of state organization.

Washington, Westminster, and Whitehall provides a refreshing U.S. viewpoint on the British system of policy making. But its author is relatively uninterested in the nexus of institutional and social relationships that underpin the organizational dynamics of Parliament and Whitehall. Readers interested in the wider political and historical ramifications of Britain's premodern polity might want to consult Tom Nairn's *The Enchanted Glass* (1988) or Patrick Wright's *On Living in an Old Country* (1985).

KENT WORCESTER

Columbia University

The Politics of Provocation: Participation and Protest in Israel. By Gadi Wolfsfeld. Albany: State University of New York Press, 1988. 210p. \$49.50 cloth, \$16.95 paper.

In the late 1960s, when most of the world was engulfed in political protest, rebellious collective action was virtually unknown in Israel. One had to reach back to 1959 when residents of a North African slum had a brief uprising in Haifa or to student demonstrations in the early sixties when a West German chancellor visited Israel for the first time, to find examples. By the standards of the 1968 events in France or urban riots in the United States, these isolated Israeli efforts would have hardly registered on a political Richter scale.

But by the mid-1980s, Israel had become one of the most protest-prone countries in the world. Indeed, Wolfsfeld presents data on nine Western countries suggesting that it was probably number one, with only Italy offering close competition and the United States far back on the list. If the protest repertoire of the sixties seemed to bypass Israel at the time, it was only to become, belatedly, a permanent feature of Israeli political participation. In Israel today, political protest is often a first resort. How

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does one account for this startling and dramatic turnaround?

Wolfsfeld takes on this puzzle and solves it convincingly. His explanation is complex and variegated, ranging from macrocultural and structural changes in Israeli society to the nuts and bolts of collective action. He leads us through the decline of strong, paternalistic political parties from the prestate period to the late sixties and the political obstacle course that participation in electoral party politics had become (and remains).

Blocked opportunity and rising political discontent combined with a new repertoire of direct action techniques that seemed to the practitioners—with substantial objective evidence to support it—"safe, convenient, and effective" (p. 163). This politics of provocation is more nettlesome than violent and coercive. It is often "rude and disruptive but rarely uncontrollable. . . . The primary weapons are the mass media and public opinion, not rifles and guns" (p. 2). Wolfsfeld calls it "uncivil disobedience."

Using Ha'aretz, the author was able to identify almost a thousand acts of protest in the period from 1979 to 1984. Most were of short duration, with 85% lasting a day or less, but there was much unruliness, with some 150 incidents of stone throwing and 50 of burning tires. Today, these are tactics that many Israelis associate with the Intifada. But television coverage of Israeli collective action never stopped at the Green Line and is readily available to residents of the Occupied Territories (although it is not necessarily "safe, convenient, and effective" on the other side of the line).

One of the most striking features of this generally impressive book is the array of different kinds of data that the author brings to bear on the issues. He includes historical data, two national surveys, intensive interviews with a follow-up sample from one of the surveys, an events analysis based on newspaper accounts,

intensive case studies, and interviews with protest group leaders. This is triangulation at its most successful.

The Politics of Provocation is a major contribution to our understanding of Israeli politics, but it is much more than this. By exploring different types of political protest activities and identifying the constituency for each, Wolfsfeld broadens our understanding of political participation. By examining organizational variables, protest objectives, and alternative strategies, he develops resource mobilization theory and contributes to the comparative study of social movements.

The centrality of the mass media in the process is more suggested than developed. Wolfsfeld introduces the intriguing fact that television came to Israel only in 1968. Perhaps this helps to account for the surprising slowness of Israelis to adopt a style of politics that seems, by now, a central feature of the country's political culture. We are offered a conjectural paragraph on this possible influence but little elaboration. Only in the interviews with protest leaders does the media role become central enough in the analysis to add this book to the small list of works that explore the relationship between the media and social movements.

Wolfsfeld recognizes the positive aspects of the politics of provocation as a safety valve for a political system that obstructs effective institutionalized means of participation. It has severe disadvantages as well: unruliness can, and sometimes does, lead to injuries or even deaths; the resort to illegal tactics can lead to a more general contempt for the law; and such actions tend to be narrower and more reactive than institutional action. Ultimately, it is better to make ongoing participation an easier resort by removing some of the formidable obstacles to meaningful participation in electoral politics.

WILLIAM A. GAMSON

Boston College

INTERNATIONAL RELATIONS

Before European Hegemony: The World System, A. D. 1250-1350. By Janet Abu-Lughod. New York: Oxford University Press, 1989. 443p. \$35.00.

Students of world systems will welcome this book. It is by far the best general account of the state of the global economy around the year 1300, that is, shortly before the great transformation wrought by what is commonly known as the "age of discoveries" (the author disdains this term). Since classical times, the land and sea routes that are today known collectively as the "silk roads" formed the spine of the world system. They linked the eight subsystems that according to the author, connected China to Europe indirectly through India and the Mideast. Each subsystem was served by a number of cities (not only those prominent in the annals of Europe, such as Bruges or Genoa, but also Cairo, Bagdad, Palembang, and Hangchow), as the book presents succinctly yet vividly. By focusing on the network of linkages between the component geographical parts, and on the cities that served as critical nodes, Abu-Lughod brings out how a change in one region (e.g., the sack of Bagdad) affected another (the rise of Cairo). Abu-Lughod's background is in urban sociology, and she has written previously about the history of Cairo. For her, the world system is, in the first place, a network of world cities. Aided by helpful maps, the resulting perspective is a convincingly global one that accords full weight to the often-ignored productive economies and sophisticated organizations of the medieval Middle East and Asia.

But *Before European Hegemony* is not just an attractive portrayal of the state of global interactions some seven centuries ago. It carries a message and is intended to teach lessons of interest to political scientists: "The world economy of the 13th century contained no single hegemonic power" and therefore provides not only "an important contrast" to the Europe-dominated world system that followed, "whose players were interested in short-term plunder rather than long-term exchange," but

also a model for "future world systems."

Let us immediately agree that world systems are not invariant and that there need be no privileged way for its parts to coalesce. World systems unfold and develop, and much is to be learned by political scientists about the patterns of that evolution. But having said that, how are we to take the thesis that the thirteenth century was a kind of Golden Age of the world system, a peaceable world free of hegemony with "a large number of players whose power was relatively equal" (p. 361)? So why did such an admirable system collapse within a few decades? Abu-Lughod's only real explanation for that is the ravages of the Black Death of 1348.

In my view, this rosy perspective on the thirteenth to fourteenth centuries (followed by the bad dream of European hegemony) is plain wishful thinking. Here was a moment in time when the world came the closest ever to the establishment of a world state by conquest. The Mongols under Kublai Khan ruled over one-third of the world population, a proportion never attained before or since by any empire. Notwithstanding Marco Polo's glowing reports, the *pax mongolica* was in fact imposed by atrocity and maintained by terror. The campaigns of Ghengiz Kahn devastated large parts of the Middle East and significantly depopulated China (according to some, by one-half). After the sack of Bagdad in 1258, and the demise of the central silk route, the northern route was opened only soon to become a conduit for the Black Death.

As the Mongols' flawed order moved inexorably toward catastrophe, the southern route flourished for a while under the control of the Mamluks of Egypt, who had mounted a successful stand against them. Might this be the example of a multipartner, free market economic order, superior to the one the Portuguese put in place after 1500? I do not think so. The political foundation for the pre-1500 regime was the defeat inflicted on the crusader states and the command of the Red Sea established by Saladin after 1182 (p. 228). On that basis, the trade in spices was operated by

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Egypt's Karimi merchants, a cartel of long-distance traders, from which non-Moslems were excluded (even though the Venetians later operated the European end of it). Little wonder, then, that this notoriously lucrative monopoly would attract competition that ultimately succeeded in destroying it. To dismiss the Portuguese, who competed successfully, as "intruders" implies that Mamluk-Karimi monopoly was preferable to that of the king of Portugal. Furthermore, to assert that Portuguese activities were short-term plunder unrelated to long-term exchanges is unacceptable. Right from the start, these activities had the aim of replacing the Moslem monopoly by a Portuguese control over an essential commodity. From a world perspective, the effect was to invigorate the spice trade and, in the long run, to reduce the price to consumers.

In short, the world of 1300 had plenty of hegemony and can claim little, if any, moral superiority over the European kind. Readers may want to ignore the author's "lessons," but that should not stop them from using and enjoying this otherwise illuminating work.

GEORGE MODELSKI

University of Washington

Ruling the Waves: The Political Economy of International Shipping. By Alan W. Cafruny. Berkeley: University of California Press, 1987.

International organizations are usually established in the name of a purported need for stability, used primarily for redistributive rent seeking and defended as the most efficient means of allocating resources. Cafruny shows that the norms governing international shipping are little different in both their nominal and their real goals. He sets out the history of international shipping since the seventeenth century and analyzes the political aspects of the market structures that have evolved since the late nineteenth century, focusing on the European-based cartels, the role of the United States as a rising hegemon, and the efforts of the developing countries to use United Nations Conference on Trade and Development

(UNCTAD) to skew the redistributive aspects of the system toward themselves.

Following an introduction on why he believes the hegemonic stability framework to be a useful one, Cafruny begins his argument with a history of how Britain took control from the Dutch and implemented a regime based on freedom of the seas. As British power declined in the late nineteenth century, European cartels began to manage the system (on the grounds that competition would be destabilizing!) and set profit-maximizing freight rates at a lower overall shipping output.

The rest of the book is devoted to analyzing the shipping regime after World War II. As in the case of international finance (the Bretton Woods system) and international trade (the International Trade Organization and General Agreement on Tariffs and Trade), U.S. "liberal internationalism" clashed with European "national capitalism." As in these other two cases, the United States mostly gave in, allowing a regime to be set up that reflected the principle of privatization (i.e., shipping cartels, called "conferences"). Formal international organizations (such as the International Maritime Consultative Organization) were politely but firmly shunted off to the sidelines to discuss marginal technical issues.

Cafruny discusses how the industrial organization of shipping differed markedly between the bulk (raw materials) and the liner trade. In the bulk trade, U.S. multinationals (often simultaneously owners of the materials being transported) took control from the Europeans by using flags of convenience to offset higher U.S. labor costs. This, Cafruny suggests, validates hegemonic stability theory, since the United States needed to control supplies of raw materials. In the liner trade, the United States left the European cartel intact, ostensibly to help postwar recovery. By the middle of the 1960s, however, the United States was acquiring a large market share of the liner trade, based both on government subsidies and on the technological innovation of containerization, which U.S. shippers apparently adopted at a faster rate than the Europeans. This theme is continued as Cafruny shows how once the United States had achieved a strong position in the liner trade, it sought to maintain, rather than break down, the cartel. In furtherance of this end, the Reagan administration obligingly passed the

U.S. Shipping Act of 1984, which restricted the application of antitrust law.

The role of developing countries is explained in chapter 6. As in other areas of international economics, they claim to be exploited and argue for a more efficient regime. What they have asked for, in UNCTAD's code of conduct for international shipping, is actually a much more tightly organized cartel in which they have a larger share of the cartel rents via control over freight rates and market shares. Anyone familiar with UNCTAD's demands in the areas of trade and investment will be in familiar territory here.

My only general complaint is that the analytic framework is weak. The main hypothesis (hegemons stabilize markets) and its subsidiary hypothesis (when the hegemon is in a state of decline, domestic politics will strongly affect its propensity to undermine the regime) are so vaguely specified as to be untestable, particularly in the context of a single case study. The deductive bases of the hypotheses are not entirely clear. Why, for example, should domestic politics be important in declining, but not in rising, hegemons? How does Cafruny know that the European shipping cartel promoted postwar recovery (especially given the small economic costs generated by the cartel)? He sometimes seems to confuse regime stability with efficiency. For example, he states that the postwar liner cartel was an "efficient" means of transporting world trade (p. 136). The hypotheses seem to be more in the nature of case observations than deductive theoretical modifications of hegemonic stability theory.

Cafruny does not address the magnitude of importance of the shipping issue. Using data from the book, imperfect competition in the liner trade between the United States and the rest of the world created, in 1963, insignificant deadweight welfare losses of about \$273 million and redistributed about \$778 million from consumers to producers. If the developing countries think that they are in for a major treat once they get their fingers into the cartel pie (if they do) they are surely in for a big disappointment.

Regardless of whether one is happy with the regime framework, this is a rich and insightful book that reflects an enormous amount of research. It will be useful not only to those interested in shipping but also for the important parallels that exist between this issue area and

others in the postwar international political economy.

JOHN A. C. CONYBEARE

University of Iowa

Reluctant Realists: The Christian Democrats and West German Ostpolitik. By Clay Clemens. Durham: Duke University Press, 1989. 369p. \$48.75.

Reluctant Realists rounds out the established literature in English on West German foreign policy, joining William Griffith's *The Ostpolitik of the Federal Republic of Germany* (1978) and Angela Stent's *From Embargo to Ostpolitik* (1981). The book examines two previously overlooked sister parties, the Christian Democratic union (CDU) and Christian Socialist union (CSU) and takes the subject well into the present decade. Since the CDU-CSU, or the Union, shed its opposition status in 1982 to head a new Center-Right coalition government and has therefore had to grapple with the legacy of *Ostpolitik* as a party in power, the book is a welcome addition to the literature.

Clemens traces the reluctant journey of the CDU-CSU from die-hard opponent to custodian, even protagonist, of *Ostpolitik*. The social democratic-liberal opening to the East, launched in the early 1970s, marked a permanent departure from the country's defensive, Western-oriented foreign policy, and sent shock waves through the CDU-CSU that continue to reverberate to this day. *Ostpolitik* provoked a virulent Union response that originated in the party's traditional position on German reunification and East-West relations. Union reformists and fundamentalists alike expressed grave concern that the policy, however well intentioned, could result in the permanent division of the two Germanies, the Finlandization of West Germany, or (at a minimum) isolation from its European and U.S. allies. Yet over the course of the next decade, the Union reversed its position. In fact, during Kohl's tenure as chancellor, intergovernmental contacts between West Germany and its eastern neighbors, particularly the GDR, are up; citizen visits and cultural exchange programs have mushroomed; and economic ties have deepened. How to explain this?

According to Clemens, the long march of

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the Union flowed from a combination of international, societal, and intraparty factors, with causal weight assigned to the first two. The consolidation of the international bloc system, combined with détente, rendered the Union's position increasingly anachronistic. Not only was the CDU-CSU out of step internationally but Brandt's *Ostpolitik* captured the electorate's imagination and bequeathed a sense of normalcy to West Germany's international profile. Brandt's domestic fait accompli compelled Union politicians, languishing in the political wilderness since 1969, to adapt. Most intriguingly, the limits of *Ostpolitik*, exposed during the Schmidt era, "enabled the Union to live with lingering doubts about the possible long-term impact" of the policy (p. 5). By the early 1980s, the CDU-CSU had fashioned an "all-weather *Ostpolitik*" (p. 273) that accommodated the dualistic, even contradictory, tendencies within the two sister parties.

Reluctant Realists, which draws on the extensive German literature as well as on internal party documents and interviews, is not a book driven by theory. Indeed, the author himself characterizes it "as a piece of political history, a story" (p. 7). This approach offers several advantages. Above all, it allows Clemens to portray in convincing fashion the entrenched intraparty fault lines that prevented a rapid, formal reversal of the Union position. The power of the fundamentalists, with their deeply rooted yet equivocal commitment to German reunification and their strong ties to expellee groups, comes through in vivid detail. By documenting the twists and turns of party debates over *Ostpolitik*, the author's main explanation for the eventual triumph (perhaps a better term would be *resilience*) of the reformists is that much more compelling. Subject to immutable international constraints—above all the hardened division of Europe—*Ostpolitik* by the late 1970s had lost its threatening quality, and Union reformists were able to carry the day (admittedly by stealth) within party ranks.

The rich, narrative approach also clarifies why the Union has yet to formally endorse the principles of *Ostpolitik*. Clemens searches in vain for the foreign policy equivalent of Bad Godesberg. In 1959, the Social Democratic party met in this quiet suburb of Bonn to consider its dismal electoral future. Facing permanent minority status, the party jettisoned its

socialist heritage, embraced the social market economy (until then the sole property of the Christian Democratic and Liberal parties), and ushered in the German postwar domestic consensus. During the tortuous adaptation of the CDU-CSU to *Ostpolitik*, a Godesberg never materialized. For Union reformists, the risks of adaptation subsided over the period in question; but the risks of internal fracturing within the party did not. "Pragmatic dualism" is the result. Thus, the Federal Republic remains one of the few Western democracies in which the Left has established the prevailing consensus on foreign policy; but it is a consensus acknowledged with a wink and a nod, not an open embrace, by the other side.

The political historical approach is not without its drawbacks. Clemens argues that international and domestic factors combined to force the recalcitrant arrival of the CDU-CSU as *Ostpolitik*ers. He argues that all constraints are not created equal; that is, the loosening or tightening of constraints at the international level were critical in sending the Union down its preordained path. However, the author's reluctance to explore the theoretical interaction between international and domestic factors leaves one hard-pressed to explain—let alone anticipate—future developments in this highly volatile area. Much has happened since this book went to press: the death of Franz Joseph Strauss, the rise of the right-wing Republikaner party (not unrelated to Strauss's passing), the spread of *glasnost* beyond the borders of the Soviet Union, and the sudden influx of East German refugees and resulting leadership changes there. These have transformed the constraints within which Union politicians pursue their unsynthesized *Ostpolitik*. As the division of Europe begins to look less firm, the issue of German reunification has once again emerged as a topic of serious international and domestic debate. Yet the Union response, led primarily by the fundamentalist wing, has been propelled not by adjustments to changing international constraints but by internal and competitive party politics, namely, the need to reintegrate right-wing voters in the face of competition from the extremist fringe. *Reluctant Realists* gives us few clues that might have anticipated the heightened priority (if not primacy) of domestic party politics in shaping Union *Ostpolitik*.

This is not necessarily a failing of the book.

A political history is often ill equipped to explain the onward march of political history. *Reluctant Realists* is well argued and well written and will no doubt be required reading for anyone who hopes to speak or write knowledgeably on the subject of West German foreign policy.

JEFFREY J. ANDERSON

Emory University

American Diplomacy and the Pragmatic Tradition. By Cecil V. Crabb, Jr. Baton Rouge: Louisiana State University Press, 1989. 302p. \$37.50.

The central theses of this absorbing study are that "throughout some two centuries of its existence as an independent republic, the American nation has acted pragmatically in responding to myriad internal and external challenges" (p. 2) and that "American attitudes and actions abroad usually exhibit a high degree of conformity with the main principles and tenets of pragmatic thought" (p. 5).

Several problems limit Crabb's efforts to substantiate this thesis. The first is semantic. As Crabb himself acknowledges, pragmatism is "not a coherent philosophy but merely a disjointed collection of fragmentary, and sometimes contradictory, maxims for guiding human behavior" (p. 129). Indeed, philosopher A. O. Lovejoy was able to identify 13 separate varieties of thought that could be classified as "pragmatic" (pp. 56, 248). If there exists no intersubjective consensus about which beliefs the term *pragmatism* is meant to refer to, pragmatism's impact on policy conduct cannot be convincingly demonstrated. Pragmatic behavior, much like efforts to judge which actions are in the national interests of a state, resists classification. This being the case, to link pragmatism to practice is to load the dice; for, tautologically, one or another of these strains of the pragmatic "mode of thought" (p. 2) is *bound* to be observable in the U.S. practice of diplomacy.

A second problem resides in the author's embarrassingly brief (pp. 132-36) reliance on only three U.S. policy makers (Woodrow Wilson, John F. Kennedy, and Ronald Reagan) to support his thesis. The comparisons are neither "controlled" nor "structured" in the well-

known "focused" comparative research design advocated by Alexander L. George. The uncertainties about the representativeness of this sample, the treatment of the cases, and the weak empirical foundation of the evidence reduce confidence in the cogency of Crabb's thesis.

Third, the existence of a causal linkage cannot be established by the methodology employed. To confirm his theory, Crabb recommends that it be "capable of empirical verification by reference to the empirical experience of the United States over an extended period of time" (p. 144). But fragments of the historical record selectively referred to do not provide an adequate empirical base for theory testing. This approach does not permit falsification. Selective illustrations, moreover, could be used to support any number of rival propositions about the most accurate way of characterizing the U.S. diplomatic tradition. For example, consider the contending characterization that Crabb himself previously advanced in *The Doctrines of American Foreign Policy* (1982), wherein he contrasted the traditionally pragmatic preference of U.S. citizens "in their internal affairs" "for nondoctrinal approaches to public policy issues" with the alleged fact that "no nation in modern history has relied so heavily upon 'doctrinal' statements and principles in foreign affairs as the United States" (pp. 1, 2). We might justifiably ask, Which of these incompatible diplomatic traditions—a pragmatic or a doctrinaire one—is the more characteristic or dominant? Under what conditions?

Finally, but not exhaustively, another threat to valid inference is created by an approach that necessarily generalizes from the attributes of aggregates to individuals (and vice versa). To investigate the influence of pragmatic beliefs on individuals' foreign policy performance, Crabb must delineate both the beliefs of a collectivity and the beliefs underlying the decisions of individual policy makers within it, in order to link the two causally. In an approach that makes observations at one level of analysis and infers patterns at another there is a high probability of committing an ecological fallacy (which would considerably weaken the strength of his thesis).

The book's central conclusion, that U.S. foreign policy making is most effective when guided by pragmatic calculations of national interest, is neither novel nor surprising. In-

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deed, it is consistent with the conventional wisdom posited by orthodox realpolitik logic, which takes as its point of departure the premise that states' interests are best served when decisions are made by hard-headed leaders through cost-benefit calculations of national advantage. Defending pragmatism is like defending rationality or adaptability; pragmatism is an elusive aspiration of which nearly every one approves. Some readers might cynically ask if Crabb explores a "school of philosophy" (pp. 6, 83) that no one questions.

But that criticism would be unfair. While Crabb's extremely well written account argues that the advantages of a pragmatic approach far outweigh the disadvantages, he acknowledges also that a pragmatically grounded diplomacy "unquestionably has shortcomings and defects" (p. 271). As John G. Stoessinger's *Crusaders and Pragmatists* (1985) suggests, limitations exist to any pragmatic, purely expedient approach that is not restrained or inspired by respect for an idealist vision, even if relatively greater success and less failure may have resulted when U.S. foreign policy has been managed by pragmatic leaders than when it has been managed by crusading leaders.

Crabb's impressive scholarship seeks to take theorizing beyond the narrow channels in which existing explanations of U.S. diplomacy have been framed. Those frameworks, he maintains, are inadequate; we continue "to lack an overall theory of [U.S.] foreign relations that coherently integrates its diverse diplomatic activities" (p. 272). Crabb's major contribution lies in his heuristic suggestion of a possible approach to bridging the gap between realism and idealism and improving the explanatory power of theories of U.S. foreign policy behavior. Could it be, as Crabb contends, that "a pragmatically based explanation serves better than nearly all other available theories to make the international behavior of the United States intelligible" (pp. 143-44)?

Unfortunately, the reader seeking the evidence necessary to place confidence in this assertion is unlikely to find it in Crabb's presentation. Nor has a compelling theoretical case been built for the prediction that "it may be confidently anticipated that, under any incumbent administration, the foreign policy of the United States will reflect the American society's belief in the leading tenets of pragma-

ticism" (p. 137). The challenge for scholars is how to test these assertions, qualify and refine them, and broaden the inquiry so that the external behavior of the United States can be better explained and predicted.

CHARLES W. KEGLEY, JR.

University of South Carolina

The Falklands, Politics, and War. By G. M. Dillon. New York: St. Martin's, 1989. 284p. \$45.00.

The Sovereignty Dispute over the Falkland (Malvinas) Islands. By Lowell S. Gustafson. New York: Oxford University Press, 1988. 268p. \$36.00.

These two studies of the 1982 Falklands (Malvinas) War provide the reader distinct approaches to a subject deserving careful scholarly analysis. The Dillon book confines itself to the task of explaining with great clarity how Thatcher, Carrington, and other high-level Whitehall leaders, by following a dangerous course of political immobility prior to the Argentine invasion of the islands, were in great measure responsible for the tragic consequences of the conflict. According to the author, from May 1979 when the Conservatives took office, the pattern of political indecision about the Falklands eventually led to a discontinuation of sovereignty negotiations in January 1981. Ironically, the prime ministerial neglect of crisis management and the outbreak of hostilities unexpectedly and quickly intertwined domestic politics with foreign policy and resulted in a political victory for the government. Dillon attributes this phenomenon to public sentiment traced to Britain's experience with the heroic Churchill years and a nostalgia for the empire. Thus, the concept of Fortress Falklands engendered by the dramatic dispatch of the Royal Navy Task Force precluded any hope of a diplomatic solution, especially after the sinking of the Argentine ship *Belgrano*.

Major strengths of this book are the lucid style used to analyze a series of extremely complex maneuvers by decision makers during the years of searching for a viable solution to a foreign policy dilemma and the in-depth documentation of the incremental decisions by British officials and bureaucracies in trying to arrive at a solution to the controversy over a dis-

tant postcolonial anomaly in the South Atlantic.

An important lesson to be learned from Dillon's thesis is that a seemingly insignificant dispute over the future of nonstrategic islands inhabited by 18 hundred people can rapidly fuel the flames of nationalism, while politicians and statesmen are overtaken by events beyond their control. In a world saturated with deadly weapons of war, no situation should be allowed to escape the search for diplomatic solutions. In an open, democratic system where numerous rational options were debated in the House of Commons, the Foreign Office and the Cabinet, the long-term indecision proved to be the nemesis of peace. Dillon convincingly argues that while war distorts and controls politics, politicians should be constantly vigilant in their pursuit of peaceful and compromising foreign policies and never allow conflict with other nations to become volatile due to indifference toward negotiated settlements.

The material in this volume should be of great interest to students of British public policy who want to know more about the strains between foreign and defense policy demands and domestic politics. The appendix, notes and bibliography indicate how skillfully Dillon used sources available to him to reconstruct the conduct of decision makers when war and politics clashed in a democratic system. He admits that a lack of knowledge of the Argentine political process precludes him from making judgments about military policy in that nation leading up to the war. Readers will have to consult other available volumes to have access to material that can provide a more complete overview of the Falklands (Malvinas) conflict.

The central theme of Gustafson's work is the difficulty encountered when statesmen try to define *sovereignty*, *self-determination*, *decolonization*, *aggression* and other legal terms as they apply to the status of the Falklands. Like Dillon, he argues that British vacillation in the negotiating process increased frustrations among the leaders of the junta in Argentina; but he also places blame on both sides for sending out wrong signals as to how they intended to resolve their differences. In this context both Argentina and Britain engaged in probing or threatening military actions before the full-scale Argentine invasion of the islands. The armed forces of both nations were woefully unprepared to fight a limited war; al-

though young conscripts, officers, and politicians on both sides were encouraged by the exhilaration displayed by their fellow citizens for engaging in hostilities. The writer proposes that the single greatest stumbling block to a negotiated settlement was the future of the Falklanders, or "kelpers." Gustafson shows how the Anglo-Saxon kelpers won favor with the British Parliament through a well-organized lobbying group stressing self-determination for the islanders.

By contrast, Argentina, working within the framework of the United Nations, searched for an analogous international situation to support her argument that the British control of the islands was an outdated remnant of colonialism and imperialism. To win favor with the Third World anticolonial and nonaligned nations Argentina's representatives used India's reconquest of Portuguese Goa in 1960 as an acceptable use of force to undo the injustices of the colonial age. However, Gustafson explains that in the 1980s African and Asian nations were more restrained in their anticolonialism and more attuned to principles of self-determination of the islanders. Thus, the use of force by Argentina was not perceived as a war of liberation of their own people by the Third World members of the United Nations.

In the late 1970s and early 1980s one basis for negotiating the sovereignty issue was founded upon joint Argentine-British exploitation of oil resources in the South Atlantic. This seemed to be a promising prospect for diversifying the monoproduktive economy of sheep raising on the islands. The success of the North Sea drilling for Britain was evidence for a mutually beneficial venture for the governments in Buenos Aires and London. The British had the technology for offshore drilling and could use economic cooperation to open discussions on the issue of sovereignty acceptable to both Argentina and the Falklands. Hardliners in Buenos Aires, according to the author, perceived economic cooperation as a stalling tactic and encouraged the Argentine state oil company Yacimientos Petroliferos Fiscales (YPF) to begin offshore exploration and leasing in the waters close to the Malvinas (Falklands) unilaterally. At this point sovereignty negotiations halted.

Gustafson does an excellent job of dispelling the common belief concerning the outbreak of the war that both General Galtieri and Prime

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Minister Thatcher actively pursued hostilities to call attention away from domestic problems in the two nations. The "externalizing conflict" theory is too simplistic as an explanation for the war.

Both books present detailed examinations of the factors leading to the war and reflect on the future consequences of the conflict. In combination the two volumes provide balanced and objective perspectives on the complexities of a topic that has previously been covered only in a fragmented fashion.

ALAN T. LEONHARD

University of New Orleans

To Make a World Safe for Revolution: Cuba's Foreign Policy. By Jorge Domínguez. Cambridge: Harvard University Press, 1989. 365p. \$35.00.

Open societies are much easier to write about than countries like Cuba. Critics need to take this into account in comparing books on open, with books on closed, societies. Jorge Domínguez' command of Cuban affairs is so firm that he almost seems to achieve the penetration usually possible only in open societies.

The author's use of published sources, bolstered by interviews, is impressive. Much is gleaned from books, magazines, newspapers, hearings, speeches, bulletins, and pamphlets. There are 55 pages of notes, almost exclusively citations. The book is a gold mine of facts, figures, names, dates, and events across the full spectrum of Cuban foreign relations. But it is not a book for a typical undergraduate nor for the general reader lacking special interest in Cuba. Domínguez has crammed in so many facts on so many different subjects as to rule that out.

The special value of his work comes from its comprehensiveness and attention to detail. Individual chapters are devoted to U.S.-Soviet-Cuban relations (the "security regime") and Cuban relations with the USSR, revolutionary movements, revolutionary states, capitalist countries, and the Third World. Domínguez will not tell the specialist much new about the general contours of Cuban foreign policy, but he does supply in a well-indexed volume evidence and analysis on hundreds of issues that are difficult to find anywhere else.

Here and there the author whets the reader's appetite for more. One such case is his report of a proposal that Cuba, with Soviet consent, commit itself to stay out of a war between NATO and the Warsaw Pact (p. 57). In that eventuality, the United States would not attack Cuba. Implementation would be assured in the event of hostilities by Cuba's keeping its aircraft on the ground and its submarines out of water in designated ports. The United States would keep its aircraft on the ground in southern Florida and Guantanamo. Citing his interviews in Havana on another issue, he also reports that Cuba and the United States started to negotiate exchange visits between nuclear plants, one in Cienfuegos and another in the United States.

One of the book's strengths is its discussion of Soviet-Cuban relations, particularly economic relations. Domínguez views Cuba as falling under Soviet hegemony, grateful for, and dependent on, Soviet assistance but hesitant about acknowledging that dependence. Having gone to press earlier in 1989, the manuscript could not have taken into account the epoch-making events in the Soviet Union in the latter half of the year. Although data is not yet available, it now appears likely that there will be sharp cuts in Soviet aid, which, together with many other factors, could lead to a major reorientation of Cuban domestic and foreign policy. His book may have come at the end of an era.

This volume contains no concluding summary or chapter that rounds up the author's major findings. His last chapter, devoted to how Cuban foreign policy is made, serves as a partial and engrossing substitute. Throughout the book Domínguez capitalizes on his data to make interpretations and draw broad conclusions.

I am struck by how little we understand about why the United States has behaved toward Cuba as it has and why the USSR was so generous to the Castro regime. Similarly, What has been the role of domestic forces in shaping U.S. policy? What were the interests inside the Kremlin that made massive Soviet aid possible? If some future authors explain those subjects as well as Domínguez has explained Cuban policy, we shall be most fortunate indeed.

COLE BLASIER

University of Pittsburgh

Managing International Markets: Developing Countries and the Commodity Trade Regime. By Jock A. Finlayson and Mark W. Zacher. New York: Columbia University Press, 1988. 348p. \$40.00.

Although a great deal of attention has been devoted to the relatively few Third World countries that have achieved significant gains from trade in manufactured goods, most underdeveloped countries continue to rely on a more traditional mode of trade, the export of primary commodities. The importance of commodity trade to the Third World is indicated by its prominence on the South's agenda in negotiations with developed countries in a variety of international forums over the last four decades.

The central contribution of Finlayson and Zacher's book is a comprehensive review of North-South commodity bargaining over the entire postwar period, which address a real gap in the literature on the topic. Although a number of recent studies have discussed problems of commodity trade in general or have considered individual products, none has examined the entire range of primary products of interest to the Third World, focusing on the political as well as the economic aspects of commodity bargaining. *Managing International Markets* is certainly the most comprehensive discussion of North-South commodity negotiations published in the last decade, and it will remain an essential source on the topic for years to come.

Finlayson and Zacher's intention is to contribute not only to the literature on North-South commodity bargaining but also to the broader theoretical literature of international political economy. In particular, they explore the notion that poor and weak international actors tend to favor political rather than market outcomes, which in the area of commodity trade means the construction of a managed regime more congenial to Southern interests than the existing market-based regime. Third World efforts to reform the global commodity regime can be traced to the 1950s, but they gained momentum with the inauguration in 1964 of the United Nations Conference on Trade and Development (UNCTAD), which became the key forum for North-South discussion of trade issues. UNCTAD's efforts culminated with its Integrated Program for Commodities (IPC), launched with high hopes in

the late 1970s. The IPC-sponsored efforts to negotiate a wide range of new producer-consumer agreements intended to stabilize the prices of individual commodities, which would be linked with existing agreements and one another by a common compensatory financing mechanism.

The inescapable conclusion of Finlayson and Zacher's analysis is that these efforts by Third World countries to create a commodity trade regime more beneficial to their interests have largely failed. In the 1980s only one new commodity agreement has been concluded; and in several cases existing agreements, some in effect for many years, have not been renewed.

Why have efforts to reform the international commodity regime been so unsuccessful? One reason, the authors argue, is that while Third World countries have demonstrated remarkable unity in endorsing the general goal of commodity management, they have found agreement more difficult when discussing individual commodities. As the authors point out, "when governments are required to sit down and hammer out an agreement that may actually affect market prices, production plans, export shares, government revenues, employment, and other tangible economic variables, their agreement on general principles may fail to prevent the emergence of sharp conflicts" (p. 260).

A second reason has been opposition from the developed countries, which are the main consumers of Third World primary products. Through numerous negotiations most developed countries have continually expressed a strong preference for market solutions over international management—a position that, as Finlayson and Zacher point out, seems rather hypocritical in view of the fact that most developed countries offer extensive protection from price instability to their *domestic* commodity producers, particularly in the area of agriculture. In the relatively few cases where developed countries have made concessions, it has usually been for political reasons rather than because they were convinced of the inherent desirability of managed trade.

Finally, many negotiations have failed simply because of the intrinsic difficulty of regulating the markets in question. The precise reason for this varies from commodity to commodity. In some cases products are perishable and thus unsuited to the creation of buffer stocks; in

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others, trade is dominated by long-term contracts so that there is no single price structure to guide stabilization schemes; and in yet other cases products are sensitive to price competition from synthetics or are produced in many grades that are difficult to classify and regulate. Whatever the specific reason, price stabilization schemes often confront economic obstacles that reinforce the political obstacles discussed above.

All of these factors and many more are addressed in Finlayson and Zacher's analysis, which makes a valuable contribution both to the literature on North-South commodity bargaining and to the broader theoretical literature of the field of international political economy. Either contribution would have been welcome, but the authors' success in linking the two makes *Managing International Markets* an especially impressive achievement.

VINCENT A. MAHLER

Loyola University, Chicago

NATO's Conventional Defenses. By Stephen J. Flanagan. Cambridge, MA: Ballinger, 1988. 172p. \$34.95 cloth, \$14.95 paper.

The Politics of European Defense Cooperation: Germany, France, Britain, and America. By David Garnham. Cambridge, MA: Ballinger, 1988. 224p. \$29.95.

After forty-five years the invasion of West Germany from the East has finally begun. But contrary to the fears of Western defense analysts, it is an invasion of cars and pedestrians, not of tanks, that is joyously breaching the Berlin Wall. Unfortunately, NATO's fortieth anniversary came too soon, stimulating a raft of scholarship that may now need to be reworked in the light of momentous recent events in Eastern Europe.

Works by Flanagan and Garnham respond to the question, How can Europe be defended in the coming era of declining U.S. hegemony and of European reluctance to employ nuclear weapons to fight wars? The authors respond in different ways. Garnham focuses on European defense cooperation as a way of enhancing the role European powers can play in their own defense, particularly in the management of nuclear risks. Flanagan explores the question much more broadly, examining the prospects

for evolving a consensus on more effective strategies and tactics, on reforms in European conventional defense, and on cooperation. But the uniqueness of this book is his attention to technology, particularly to nonnuclear emerging technologies, as a means of maintaining an acceptable balance and of increasing stability in times of crisis.

Both works are thoughtful, clearly written, and rich in their scholarship. Both illuminate key areas of basic knowledge well. Flanagan shines in his discussion of strategy, notably of different conceptions of "deep strike" strategies including Follow on Forces Attack (FOFA) and in his brief overview of the technologies that might support them. Garnham presents an excellent succinct discussion of bilateral defense policy relations between the principal actors (the United States, Britain, France, and the Federal Republic of Germany) and offers a useful introduction to the possibilities and limitations of multilateral defense cooperation as well. Garnham's book, although it may be too basic for scholars, is a good candidate for assignment to advanced undergraduates or graduate students. Flanagan's work, as part of the International Institute for Strategic Studies series, is geared more to specialists and assumes knowledge that may stretch general readers (MLRS, for example, is never defined).

Events subsequent to the writing of these works—the Intermediate-Range Nuclear Forces (INF) Treaty, changes in Soviet military doctrine, and the startling developments in Eastern Europe—have shifted the ground considerably since these projects were undertaken. In terms of these works the developments are fortuitous, for neither author has produced a convincing answer to the basic question of how a viable defense of Europe can be achieved in the politicostrategic climate they describe. Garnham demonstrates that European Defense Cooperation has been growing but that cooperation also contains irreconcilable problems. These problems are traced to national preferences to limit coproduction for national economic reasons that include the development of national technological competence. They are also rooted in the fact that faced with any real risk of war with the Soviet Union, the Federal Republic, and Britain prefer to link their fate to the United States rather than to each other and to France. Yet Garnham concludes his book with the recommendation

that Europeans take on more of the role of the nuclear guarantor and that the United States retain a significant role in supporting conventional defense. Given political sensitivities in Germany about the use of nuclear weapons for any purpose other than deterrence, this realignment of alliance tasks seems highly unlikely. In fact, it may only prove feasible in a context of a greatly reduced assessment of the Soviet threat; for as Trevorton has wisely pointed out in a recent Council on Foreign Relations book, *Western Approaches to the Soviet Union* (1988), at very low levels of risk one becomes less finicky about the insurer. In this evolving context West Germany might even be willing to bet its fate on a "European pillar" dependent largely on a French nuclear guarantee. In this new context Garnham may be correct to dispel the fear of German "Finlandization" or "equidistance" that the French and U.S. observers associate with improved inter-German relations. Given the extreme political weakness and prospect for chaos in the East, Germany would have little difficulty deciding to stay "anchored" in the West.

Flanagan's argument is more cautious and nuanced. After reviewing NATO's forward defense strategies and arguing that emerging technologies to enhance firepower countering numerically superior forces could be very helpful, he leads us to understand that for largely technical reasons these very technologies offer little prospect of success in the near term. Flanagan wisely incorporates political and economic considerations as well. Rejecting emerging technologies as a "magic bullet," he concludes that NATO can only make incremental improvements in defense of its territory—and then only if Europeans believe that these changes will not increase their financial burden, increase the risk that deterrence will fail (as some perceive longer-range applications of FOFA may), and will not weaken the linkage to the U.S. nuclear guarantee. These are modest opportunities indeed. Fortunately, in the emerging context, even these modest gains may seem sufficient; and the fixation of the U.S. guarantee may decline as a criterion.

While timing has ultimately helped both authors' arguments, it has also been unkind in two significant ways. Both books would have been better for being able to consider from some distance the defense policy implications

of the INF agreement and the unfolding Conventional Forces Talks. Both suffer, as well, in their discussions of French defense policy, for not having had access to Richard Ullman's revelations (*Foreign Policy* [1989]) of 15 years of close Franco-American nuclear cooperation. This must certainly alter how we understand the roles French conventional and nuclear forces would likely play in a European war. Errors in timing, however, are inevitable in this fast-moving and fascinating field.

ROBERT B. CHARLICK

Cleveland State University

The Weary Titan: Britain and the Experience of Relative Decline, 1895-1905. By Aaron L. Friedberg. Princeton: Princeton University Press, 1988. 329p. \$29.95.

Aaron Friedberg has written an important book on British imperial decline that indirectly illuminates a question of current public debate—the United States' experience of relative decline. Those who are concerned either with the historical case or its present analogue will profit from the acuity with which Friedberg poses his questions and the subtlety of his historical narrative. Not everyone, however, will be convinced that he has successfully challenged the conventional wisdom that Britain reacted adeptly to the problems of imperial decline.

This conventional view, as Friedberg accurately lays it out (pp. 292-95), is that Britain clearly understood its situation of relative economic and imperial decline around the turn of the century and rationally engaged in a triage of its excessive international commitments. Britain's astute strategy of appeasing most of its potential enemies, switching from "splendid isolation" to loose alliances to contain German power, helps explain "why the British empire lasted so long"—as Paul Kennedy has put it. By implication, Britain's successful strategic retrenchment is a model for a similar strategy on the part of the United States.

Friedberg challenges this view, arguing that Britain understood the reality of its relative decline in only a halting and partial way. Due in part to this intellectual failure in measuring its power in the economic and military

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spheres, Britain chose to meet the problems of its relative decline with half measures that further undermined its power position. As a result, Friedberg suggests, the British shunned the thoroughgoing social and economic reforms that might have reversed the downward trend. Likewise, they underestimated their ability to deter German aggression through large British peacetime military deployments on the European continent (pp. 288-303).

Friedberg is a very objective commentator on British assessments of their position in four realms: trade and industrial competitiveness, finance, sea power, and the defense of India. Indeed, he is so evenhanded that he includes a great deal of evidence that heavily qualifies his overall argument. First, Friedberg's evidence does not show that Britain's head was mired in the sand. Rather, it suggests only that the intellectual grasp of the reality of relative decline proceeded at different rates in different areas and that residual ambiguities allowed observers to disagree about the magnitude of that decline. Indeed, only in the area of trade were these ambiguities very significant.

Second, Friedberg's contention that erroneous power assessments were a key "intervening variable" explaining Britain's allegedly inadequate policy response is not strongly demonstrated (p. 290). In virtually every issue area, it emerges that the problem of fashioning a response was not intellectual but political. Conservatives were unwilling to increase taxes on elites in order to pay for an expanded army (131-33). Most liberals and consumers were unwilling to reorient a system of free-trading political economy that served their interests. They generally understood that Britain was experiencing a *relative* decline, but they cared more about Britain's—and their own—continuing *absolute* increases in wealth (pp. 79-81). Moreover, policy responses in different issue areas were poorly coordinated not so much for intellectual reasons as for reasons of bureaucratic fragmentation (pp. 280-82).

Finally, it is not demonstrated that Britain's response to decline was clearly inferior to any available alternative. Friedberg admits, for example, that naval retrenchment and the new alliances "may have been unavoidable" (p. 208) and that protectionism was no solution (p. 84). Consequently, a good deal of the conventional wisdom survives Friedberg's

critique.

Nonetheless, Friedberg asks incisive questions and demonstrates historical scholarship of a very high quality. His book is well worth pondering in a period of U.S. adjustment to what Paul Kennedy has called the problems of "imperial overstretch."

JACK SNYDER

Columbia University

Soviet Perceptions of the U.S. Congress: The Impact on Superpower Relations. By Robert T. Huber. Boulder: Westview, 1989. 197p. \$36.50.

In this careful study, Robert T. Huber, a staff consultant for the House Foreign Affairs Committee from 1978 to 1989 and currently executive director of the Social Science Research Council, sets out to identify and analyze the range of Soviet views, both specialist and leadership, on the role of the U.S. Congress in the political system and the impact of the Soviet understanding of Congress's role on the conduct of relations between the United States and the Soviet Union. The book examines Soviet views of Congress from the early 1970s through the first years of the Gorbachev period and may be seen as a case study in Soviet "learning." Soviet analysts and leaders reacted with surprise to the active intervention of the Congress (via the Jackson-Vanik Amendment of 1974, tying U.S.-Soviet trade with Soviet emigration) in the Kissinger-Ford Soviet policy, thereby precipitating a needed reevaluation of the role of Congress in foreign policy and thus of the nature of the U.S. political system.

The great merit of Robert Huber's study, in addition to his detailed exposition of the views of Soviet experts on the roles of the Congress, derives from the author's vast expertise in the workings of the Congress. He is able to judge the accuracy of the Soviet views he presents against his knowledge of the congressional system as well as the views of recognized U.S. authorities on Congress. In his overall assessment of the analyses of Soviet experts on the Congress, Huber concludes that "more often than not Americanists have produced a level of scholarship qualitatively comparable to Amer-

ican efforts on similar subjects and the trends described on many issues have been accurate" (p. 82). Most convincing for the thesis that Soviet Americanists have been engaging in genuine intellectual endeavor is the author's finding that on many key issues, such as the role of Congress in U.S. defense and arms control policy or in U.S.-West European relations, Soviet experts are as divided or confused as are U.S. students of the Congress. In consequence, Soviet Americanists "have produced a rich vein of material for which Soviet policy-makers can assess the 'Congress factor' in U.S. foreign policy" (pp. 82-83).

While the bulk of the material cited derives from the pre- or early Gorbachev period (and thus buttresses the work of Morton Schwartz, Neil Malcolm, and others, who have shown the growing sophistication of Soviet political studies in the 1970s and 1980s), Huber notes that Mikhail Gorbachev is the first Soviet leader with an apparent grasp for some of the nuances of Congress's role in U.S. foreign policy. The dramatic sight of Soviet marshals and foreign policy specialists testifying before Congress and the subtle ways in which the Gorbachev leadership has sought to work with Congress in promoting its foreign policy agenda attest to the understanding of Congress that has emerged within the USSR in the past decade and the political role that some of these specialists are now playing. Huber's book provides an able guide to the difficult conceptual path that Soviet observers have had to travel in revising their traditional, ideologically determined views of the place of the Congress and its effect on U.S.-Soviet relations.

Indeed, the reader is so impressed with the analytical core of Huber's study (pp. 18-85) that he may be forgiven for wanting to read more on the conceptual evolution of Soviet views of the Congress. The author takes great pains to discuss the relevance of, and implications for, three archetypal explanations of Soviet political conduct: the totalitarian, pluralist, and corporatist perspectives. Yet the discussion here, beyond detracting from the impressive thematic presentation, tends to blur the significance of the Gorbachev phenomenon. Since none of the three models can account for a radical reformer of the Gorbachev type, their explanatory power in terms of the evolution of Soviet views on the U.S. Congress would seem similarly limited. Otherwise, Gor-

bachev would appear as simply a more effective agent of traditional Soviet objectives in both domestic and foreign policy, a conclusion that the author himself implicitly rejects at the very end of his study in noting the tendencies at the special Party conference of June 1988 that led to a radically different role for the Soviet Union's own "Congress," the increasingly contentious new Supreme Soviet. Therefore, as the author also notes, the Soviet study of the U.S. Congress takes on added significance not only for the study of U.S.-Soviet relations "but also of the Soviet political system as a whole" (p. 173).

ALLEN LYNCH

Columbia University

The Political Economy of Japan. Vol. 2, The Changing International Context. Edited by Takashi Inoguchi and Daniel I. Okimoto. Stanford: Stanford University Press, 1988. 566p. \$37.50 cloth, \$12.95 paper.

Japan's emergence as *the* preeminent manufacturing nation has brought unsettling changes to the world and to Japan. It will bring more. Here 18 prominent Japanese and U.S. political scientists and economists address these changes, in the second of a three-volume series. (A 1987 collection treated Japan's domestic transformation; one to come will treat "social and cultural dynamics.")

This volume groups its 14 essays under broad themes like "the world system" and "Japan's emerging world role," but the chapters are best read individually. Takashi Inoguchi opens with an eclectic treatment of Japanese foreign policy, with emphasis on the economic side. Next comes a short, provocative essay by Shumpei Kumon and Akihiko Tanaka, who see global politics evolving from a prestige game to a wealth game to a (future) knowledge game. This evolution favors Japan's global position, they feel, in sharp counterpoint to Bruce Russett's conclusion in the chapter following that the continuing U.S. military, political, and cultural position renders the notion of hegemonic decline a "gross overstatement" (p. 103).

Koichi Hamada and Hugh T. Patrick analyze current and alternative world monetary regimes and Japan's role therein. Yoichi

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Shinkai treats two intertwined developments of the 1980s: liberalization of Japanese financial markets and mammoth capital exports. Japanese finance, like manufacturing before it, has "outgrown Japan's political and military status" (p. 271). For Robert G. Gilpin, today's international trade regime features unprecedented commercial interdependence, but "new protectionism" and economic nationalism threaten its long-term survival.

Several other essays also discuss trade, but with emphasis on Japanese practice. With her seemingly closed markets, is Japan an outlier? Gary R. Saxonhouse says *no*, deploying in support of this conclusion his well-known model attributing Japan's modest manufactured goods imports to the structure of her comparative advantage. Ryutaro Komiya and Motoshige Itoh reach a similar verdict. What they find in need of explanation is Japan's repeated willingness to make trade concessions to the United States! Daniel I. Okimoto is more skeptical. Applying his model of "political inclusivity," he finds that Japan's trade-policy making structure poses "deep-seated difficulties in integrating Japan into the international system" (p. 340). On the export side, Peter J. Katzenstein finds that Japan's strategy "often aims at total domination" of markets (p. 302). This is one of several reasons why Japan cannot become the Switzerland of the Far East, Douglas MacArthur notwithstanding.

Moving to political-strategic matters, Donald C. Hellman sees little change. Current elitist politics and institutions render Japan incapable of playing "an expanded and significant role in international politics" (p. 371). He sees this as bad. Charles E. Morrison similarly finds, concerning Southeast Asia, "little [Japanese] willingness or capability to develop an independent role" (p. 444), but he sees this limitation as good, supporting mutually beneficial U.S.-Japan regional cooperation.

Looking to future prospects, Stephen D. Krasner finds "no substantial evidence that the growing economic interchange . . . is any guarantee of stability" (p. 410). He concludes here (as elsewhere) that U.S. trade policy should shift from enforcement of broad, liberal goals to specific reciprocity, a "web-of-contracts" built on "interests in specific issue-areas" (p. 413). And in a stimulating finale, Kenneth B. Pyle looks both backward and forward in linking four prominent "schools of thought" con-

cerning Japan's role: the "progressive view," the "merchant-state thesis," the "realist school," and the "new nationalism" (p. 457). Discussion of the twenty-first century is "extensive" in Japan and "remarkable for its optimism" about Japanese fitness for the coming "information society" (p. 484).

As is typical of such volumes, the parts form less than an integrated whole, even though the editors' introductory summary poses the central questions admirably, sometimes taking the logic of authors' analyses a step or two beyond the chapters themselves. The end product will not satisfy—and does not persuasively refute—holders of what is becoming the scholarly, as well as the journalistic, consensus, namely, that Japan's market is structurally resistant to imports. Nor could a volume several years in the making have addressed the sweeping claims of the "new revisionists"—James Fallows, Clyde Prestowitz, Karel G. van Wolferen—who see Japan as a culturally alien economic juggernaut that the West must somehow "contain." What the authors have given us are essays richer than can be summarized here, variations on a theme that will challenge leading scholars—and national policy makers—for many years to come.

I. M. DESTLER

University of Maryland

Alliance under Tension: The Evolution of South Korean-U.S. Relations. By Man-woo Lee, Ronald D. McLaurin, and Chung-in Moon. Boulder, CO: Westview, 1988. 229p. \$35.00.

The transformation of South Korea from a poor, war-shattered country into one of the top 10 trading nations of the world in the span of a few decades is a remarkable story, which has justifiably attracted the attention of scholars and journalists alike. Given its importance, the special relationship that has been forged between the United States and South Korea has emerged as a major focus of research, generating a succession of monographs and anthologies in recent years.

Alliance under Tension by Lee, McLaurin, and Moon is one of the latest additions to the growing list of English-language books on Korea-U.S. relations. Theirs is neither a mono-

graph nor an anthology, although it comes close to being the latter. The book consists of nine chapters, all written by single authors without consulting others, except for one two-author collaboration. In short, this is a collection of essays rather than a coherent treatise on the political, economic, and security dimensions of Seoul-Washington relations. The essays vary not only in style and approaches but also in analytic rigor and scholarly content.

Even though all of the essays deal with important topics, those dealing with anti-Americanism and trade friction between South Korea and the United States are particularly noteworthy. In an essay entitled "Anti-Americanism and South Korea's Changing Perception of America," Man-woo Lee presents a penetrating analysis of the ideological underpinnings of anti-Americanism. Drawing freely on personal observations and interviews with students and politicians in South Korea, Lee identifies four major themes in what he calls the "phenomenon of South Korean anti-Americanism" (p. 25).

First, it has a Marxist overtone; and the ideology of *minjung* (masses) to which some of the radical students subscribe, bears a striking resemblance to the ideology of *chuch'e* (self-reliance) championed by North Korea. Second, the phenomenon has a strong instrumentalist orientation in that the student activists in the anti-American movement are committed to the goals of democracy and economic justice. The apparent contradiction between these two themes is emblematic of the fractionization of the movement. A third theme is the use of the United States as a scapegoat for Korea's problems and troubles. Finally, anti-Americanism is linked to the resurgence of nationalism in South Korea.

"Irony of Interdependence: Emerging Trade Frictions between South Korea and the U.S." by Chung-in Moon is one of the most informative analyses of this increasingly important topic available in English. Even "experts" in Korean studies are likely to find Moon's discussion of the legal, institutional, political, and economic framework of the trade friction illuminating. After dissecting both "macrolevel" and "microlevel" sources of the friction, Moon examines five possible strategies for coping with it: (1) positive adjustment, (2) trade diplomacy, (3) issue linkage, (4) evasion, and (5)

transnational lobbying. He subjects each of these strategies to cost-benefit analysis, concluding that a mix of all of these options except evasion holds the best promise for Seoul.

The remaining essays deal with the relative weight of security and human rights in U.S. policy toward South Korea (Lee), "U.S. Third Country Arms Sales Regulation and the South Korean Defense Industry" (Moon), the South Korea lobby in the United States (Moon), the structures and processes of U.S. foreign policy making toward South Korea (McLaurin and Moon), United States-South Korean security relations (McLaurin), and an overview of U.S.-South Korean relations (McLaurin).

While the essays authored by Ronald D. McLaurin complement the other essays by injecting Washington's perspective into the equation, they nonetheless suffer from a few minor flaws, of which the most notable is an overreliance on secondary sources. One of the sources on which McLaurin relies heavily, Norman Jacobs's *The Korean Road to Modernization and Development* (1985) is based almost entirely on secondary sources. Hence, part of McLaurin's generalization is based on what amounts to a tertiary source.

On balance, however, the volume under review adds new insight, as well as some provocative ideas, to the literature of Korean-U.S. relations.

B. C. KOH

University of Illinois, Chicago

War and Peace in Central America: Reality and Illusion. By Frank McNeil. New York: Charles Scribners' Sons, 1988. 310p. \$19.95.

The book is partly a primer on Central America but mostly a memoir by a decent, intelligent diplomat, who retired after a series of bureaucratic skirmishes with then assistant secretary of state under the Reagan administration, Elliott Abrams. The book is a product of pique from a man whose job as a self-effacing foreign service officer for 30 years, had put a premium on privacy and discretion. Putting aside diplomatic niceties, McNeil openly aimed the book at Abrams, writing it as a "caution against substituting fanaticism for facts in foreign affairs." McNeil also recognizes that the

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book is not for "historians" but for the general reader who might want to read "a firsthand account of how we got into such a mess in such a small place" (p. 10).

War and Peace in Central America begins with a breezy survey of the history of Central America in a style that is light, anecdotal, and occasionally flip. The first chapter is entitled "Welcome to Central America: An Introduction to Fantasy Isthmus." His thesis is that Central America is often distorted by North Americans projecting their fantasies and demons on the region. A *mélange*, the book offers cursory reviews of several books on Central America; reprints of McNeil's op-ed newspaper articles and those by Abrams; a travelogue of a trip McNeil took through Central America in 1987 for the World Peace Foundation; a blow-by-blow account of bureaucratic battles within the Reagan administration; copies of his letter of resignation; and his testimony before Congress. One contribution is his description of the CIA's role in using an airstrip in northern Costa Rica to help the Contras.

As a former ambassador to Costa Rica (1980-83), the author's comparative strength is his grasp of the politics of contemporary Central America, where few leaders can qualify as being all good or all bad and where internecine conflicts are endemic. "Remember, no one gets along that well with anyone else in Central America, regardless of ideology" (p. 151).

The book, however, is focused less on Central America than on the anthropology of policy making in Washington. He begins by rejecting the exaggerated notion that the United States is omnipotent: "I should make clear at the outset that at no time in Central America's history have we been responsible for everything that occurs in the region, good or bad" (p. 10). McNeil faults both the Carter and the Reagan administrations for numerous errors, a few of which are due to his describing events inaccurately. Sometimes, McNeil tilts the policies of the two administrations in a manner that makes it appear that only the author has the right balance, such as his criticism of Carter for downplaying evidence that Nicaragua was providing aid to the Salvadoran guerrillas and Reagan for exaggerating the evidence (p. 150).

In concluding the book, McNeil asserts its leitmotif: "I am struck by how the Carter and

Reagan Administrations shared certain traits, such as an inability to listen to Latin America and a reliance on rhetoric to secure foreign policy goals" (p. 276). Of course, the idea of a *single* Latin America is a cliché; each person listens to a different voice. The important point is that McNeil's book, reflecting a long tradition of retired diplomats, blames politicians and politics ("rhetoric") for bad U.S. policy, urging that the nation rely solely on "experts" (meaning career foreign service officers). At base, this is an antidemocratic argument; but it runs deeply through the work of many important diplomatic historians, including George F. Kennan.

The heart of McNeil's book, then, is the tension between the president and his political appointees and the career civil and foreign service. That tension can be creative, though it seemed mostly destructive in the Reagan administration. Instead of analyzing why and when the tension is destructive, he personalizes the struggles, implying that the problem can be solved only when political leaders stop interfering in foreign policy. Therefore, to understand the underlying issues, the reader needs to read this book alongside another that is its mirror image, Constantine Menges's *Inside the National Security Council* (1988). Menges worked for William Casey's CIA and then from 1983 to 1986 on the National Security Council staff. For McNeil, political appointees are the problem; for Menges, the immobile foreign service bureaucracy is the reason that Reagan was not allowed to be Reagan and the United States was denied his liberating policies.

McNeil avoids this stick figure analysis when he discusses Central America, but it permeates his discussion of U.S. foreign policy making. In summary, *War and Peace in Central America* is a light excursion into the complexities of Central America and a rather simple, though important, tour of an even more complicated policy-making process in Washington.

ROBERT A. PASTOR

Emory University

Resolving Disputes between Nations: Coercion or Conciliation. By Martin Patchen. Durham: Duke University Press, 1988. 365p. \$42.50 cloth, \$14.95 paper.

In a world where scholarship is often criticized for following policy, the study of conflict resolution has antedated and prepared the way for the opportunities of *perestroika* and lower-level openings for a reduction in international tensions. Patchen has synthesized much of this work in a wide-ranging presentation of the ingredients of decision making within a comprehensive framework of analysis. However, the strength of the resulting work is its weakness: Everything is there, with so many independent and intervening variables that the causal relation or explanatory power—and, in the end, the theoretical value—of any of them is tiny.

Patchen's framework is a 31-arrow flow-chart linking 16 boxes used by two parties to interact in the pursuit of conflict or cooperation (pp. 16-17), but many of these relations are further broken down in subsequent, more detailed discussions. For example, in an enumeration of conditions favoring concessions, standing pat, coercion, and rewards, over 80 circumstances are related to elements of the four decisions (pp. 157-63). The treatment is typical of the broad range of variables used to present the contextual situation (chap. 2), the parties' perceptions (chap. 3), a particularly good synthesis of decision-making theories (chap. 4), tactical choices and a detailed discussion of alternative strategies such as threats (chap. 6), arms (chap. 7), a good analysis of escalation (chap. 8), an insightful treatment of incentives (chap. 9), mixed strategies (chap. 10), and finally settlement (chap. 11). The focus is above all on superpower conflict, since the book begins with the Cuban Missile Crisis and continues to consideration of nuclear escalation.

It is only in this last section that the real theory of the work appears. It would have been useful as an organizing concept for the earlier portions: "Bargaining serves two main functions in moving disputing sides toward agreement: clarifying the situation and changing the preferences and expectations of the two parties" (p. 314). This is repeated pointedly in the summary: "To reach an agreement in a dispute between two nations, then, it is necessary to get the leaders of both sides to see the

situations in the same way, especially regarding the eventual willingness of each side to accept given terms. How can such a convergence of expectations be promoted?" (p. 339). Read backwards, the book is a complex and comprehensive answer to that question, analyzing conflict resolution as a matter of clarifying perceptions, revising expectations, and focusing on a joint outcome. Patchen makes important contributions in emphasizing the importance of uncertainty in many forms in generating conflict (pp. 295, 297, 309), in underscoring the functional aspects of conflict (p. 316), and in his treatment of positive sanctions or incentives.

There are many overlapping works, from Ikle's *How Nations Negotiate* (1964) to Raiffa's *Art and Science of Negotiation* (1982), which are not referred to in this work, and from Snyder and Diesing's *Conflict among Nations* (1977) to Pillar's *Negotiating Peace* (1983), which are. Patchen's study makes its contribution among these through its combination of social psychology and political science, which makes for rich insights but difficult theorizing. The book seeks to present an alternative to the polar debate over *si vis pacem para pacem* or *bellum* by advocating "a successful policy . . . that combines both a measure of firmness and a measure of flexibility" (p. 342). Patchen's work shows many paths and innumerable signposts, but the nature of the course means that a clear map is still hard to draw.

I. WILLIAM ZARTMAN

Johns Hopkins University

Should America Go to War? The Debate over Foreign Policy in Chicago, 1939-1941. By James C. Schneider. Chapel Hill: University of North Carolina Press, 1989. 289p. \$37.50.

This book would be an excellent collateral reading assignment in a U.S. foreign policy course. It is a case study of the evolution of public debate in Chicago over the policy responses of the United States toward the war in Europe and the role of the mass media in shaping the "reality" of events occurring abroad.

The author chose Chicago because of the diversity of its media, its heterogenous popu-

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lation, and its importance as a regional center. He explores the activities of the various interest groups in shaping the public debate. Perhaps the most interesting part of the book is Schneider's extensive analysis of how the Chicago newspapers reported the war in Europe and editorialized about how the United States should react to the conflict.

Schneider persuasively argues that media coverage tended to polarize opinion. Such coverage, "with its inherent tendency to spotlight the unusual and sensational, helped direct public attention to extremists on both sides and thus helped to foster misperceptions" (p. 220). Moreover, readers of newspapers in Chicago "received contrasting portraits of the world situation and of Washington's response to it. This fact assumes added importance because Roosevelt relied on events to educate public opinion on foreign policy. In effect, however, the public received different educational messages depending on what outlet they listened to or read. To the degree that views were shaped by the media, separate and conflicting 'realities' were formed" (p. 220).

One of the major strengths of this work is its emphasis on the grass roots operations of interest groups involved in public debate over foreign policy issues. The noninterventionists were highly fragmented compared to those who supported Roosevelt's policies. Thus, no alternatives to aid short of war really attracted much public support.

According to opinion polls in 1941, the public misperceived the situation in Europe: the majority was confident of British victory. Such an attitude was understandable, though, since it postponed the certainty of U.S. intervention. Most hoped that limited U.S. belligerency would eventually defeat Hitler; others felt that U.S. entry into another world war would be worse than a German victory.

Schneider decries the lack of national unity during 1941, but in retrospect such consensus was simply unachievable. National and local elites were sharply divided, and the majority of the public dreaded world war more than the prospect of adverse geopolitical changes in the international political system. Schneider seems to underestimate the complexity of an enduring issue in political science, namely, What is the proper role of public opinion in foreign policy?

ROBERT H. PUCKETT

Indiana State University

Restructuring American Foreign Policy. Edited by John D. Steinbruner. Washington: Brookings Institution, 1989. 260p. \$29.95 cloth, \$10.95 paper.

In introducing this collection of essays, John Steinbruner asserts that "the United States faces the probable necessity of altering the basic objectives and operating principles of its foreign policy. . . . Some hard political choices will have to be made, whether by design or default. . . . The prosperity of the next generation of Americans is in question" (p. 1).

To advance discussion of necessary changes, contributors address changes in the U.S. role in the world economy and their causes and consequences with respect to trade, exchange rate, and macroeconomic policies; a military program budget for 1990-94 that restrains spending while maintaining forces adequate to current international commitments; substantial arms reductions conducive to stable deterrence and avoiding war with the Soviet Union; reorientation of government investment in technology to further commercial competitiveness rather than in exotic military systems; the likely evolution of the Soviet and Chinese political economies and implications for the United States; and East Asian dynamics as an exemplar of a world marked by economic priorities, the emergence of multiple regional power centers, and sometimes turbulent efforts at political reform. The Arab-Israeli conflict is presented as an example of the impact of long-lived historical legacies, the need for political endeavors focused on communities and groups and not just official governments, and the possibility of programs of formal and informal U.S. nonmilitary interventions to contribute to international amity.

The essays resemble each other in that they are uniformly well informed, soundly reasoned, and readily comprehensible by the nonspecialist. They differ in other respects. Experts will find rather fresh ideas only in those on the defense budget, stability-conducive arms reduction, and the Arab-Israeli conflict. Widely held, if substantially erroneous, beliefs are systematically addressed only with respect to broad economic phenomena and the feasibility of immediate sharp cuts in defense spending. Although the limits to change posed by U.S. politics and policy processes are generally recognized, the only concrete institutional suggestion put forward is made with regard to technology investment.

More implicitly, the essays as a set reflect widespread ambivalences in current U.S. foreign policy thinking. Some authors take a position that in effect reserves judgment on the reliability of changes in the military environment; others see those changes as ripe for strong initiatives to achieve a systemic transformation. Some call for priority on domestic economic matters, (albeit with strong international implications), while others emphasize foreign policy initiatives in other policy areas. Most call for continued global U.S. leadership with more meaningful consultation yet at the same time recognize that the economic where-withal and Soviet military threat that provided the underpinnings of that leadership are no longer what they once were.

Perhaps this is only to be expected from a book that is predominantly reformist and incremental in tone, despite the bold challenge of its introduction. Hard choices are skirted as often as they are confronted, for example, with regard to free trade dogma and high-technology competition, or the possibility that others may be less than forthcoming with regard to collaboration on economic matters. Caution and a wait-and-see posture are often advocated, for example, vis-à-vis the evolution of the Soviet system, even though outcomes may be sensitive to what we do and third parties may be unwilling to stay their hand while the United States deliberates and watches. The transformational, positive programs put forward for arms reduction and the Arab-Israeli conflict are often not echoed in other contributions. Nor is there much attention to the possibility that the forecasted intensification of competitiveness in the world economy will be played out in a climate of slow, global economic growth, let alone a clear program to stimulate collective action to achieve the growth that will make trade friction relatively manageable.

We have, then, in many respects a portrait of where we are in the world; but missing is the more important issue of where we may find ourselves. Two omissions well illustrate the point. One is the focus on military crisis stability in Europe and the United States—Soviet strategic relationship. Yet we are well on our way to a world where crisis stability is more tenuous among other parties, particularly with the diffusion of long-reach weapons of mass destruction. Another is the lack of attention to

the evolution of Europe, which may well pose policy challenges that at least rival those of East Asia in scope and magnitude. Reformulating U.S. foreign policy is, as the authors recognize, a formidable task. That is all the more reason to approach it in a way that maximizes the chances that new policies will anticipate new challenges as well as adapting to ones that have already reached fruition.

DAVIS B. BOBROW

University of Pittsburgh

International Cooperation: Building Regimes for Natural Resources and the Environment. By Oran R. Young. Ithaca: Cornell University Press, 1989. 248p. \$32.50 cloth, \$10.95 paper.

In recent years the study of international institutions has shifted its focus from formal organizations to more informal patterns of cooperation or international regimes. Oran Young has been a major contributor to this transition, and this volume coherently reproduces a number of theoretical and empirical articles published and presented over the past decade.

The theoretical chapters that comprise part 1 have two objectives. First, Young seeks to place regime analysis on firmer theoretical ground by defining regimes as social institutions and "showing how our general understanding of social institutions can elucidate the conditions governing cooperation in international society" (p. 5). The first chapter provides an analytical framework for research by breaking regimes down in terms of their rights and rules, procedures, and compliance mechanisms. The conclusion (pp. 29–30) usefully organizes an agenda of questions that ought to be addressed by anyone employing a regime approach. The second chapter explicitly distinguishes regimes from formal organizations and sets up a two-by-two matrix to analyze the relationship between them. Its main contribution is its attempt to establish conditions under which anarchical cooperation or supranational authority are more appropriate. The fourth chapter reproduces Young's contribution to Krasner's *International Regimes*, in which the dynamics associated with three types of regime

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formation (spontaneous, negotiated, and imposed) are identified.

These conceptual and analytical elaborations of regime analysis have already influenced the work of scholars (myself included) and their inclusion in a single text provides a valuable resource for any serious student of international cooperation. Moreover, the simultaneous publication of the book in paperback ought to place this text prominently on the syllabi of graduate and advanced undergraduate courses in international organization.

Second, Young (particularly in chap. 3) seeks to demonstrate the explanatory superiority of a Grotian perspective (which sees regimes as pervasive features of world politics) over an orthodox or realist model, which sees institutions as mere reflections of underlying power structures because of the sharp distinction between domestic and international society. To make the case, Young marshals evidence revealing the durability of global institutions and their constraining effect on state behavior. The argument is strongest in illustrating compliance mechanisms other than centralized enforcement and in demonstrating the inappropriateness of the "public goods" metaphor in that those who attempt to "free ride" on regimes usually incur significant costs.

In parts, however, the argument presents an extreme formulation of the realist model. Moreover, some of the cases Young enlists in support of his thesis can be, and have been, explained by realist scholars, such as Waltz and Krasner. For example, the Third World's inability to alter liberal economic institutions may have less to do with the durability of institutions (p. 64) than with the fact that the balance of economic power between North and South did not change in the 1970s. While Young attributes changes in Soviet foreign policy in the 1920s to social pressures (p. 76), Waltz (*Theory of International Politics* [1979], pp. 127-28) traces it to the fact that anarchy compels states to subordinate ideology to traditional balance-of-power considerations. The inability of developing countries to challenge the International Monetary Fund (p. 72) can be explained by Marxists who see institutions as epiphenomena for very different reasons. The argument would have been more convincing had it directly engaged these rival explanations and explicitly demonstrated how one could distinguish between them. Nonetheless, Young's thesis should spark debate and stimulate further research.

The case studies on environmental and resource issues in part 2 are well integrated with the theoretical material that precedes it. This section begins by examining the disadvantages of open-entry regimes for fisheries and deep-sea-bed mining. An evaluation of regime alternatives along a variety of criteria (efficiency, equity, environmental impact) supports regional arrangements for the former and specific licensing arrangements for the latter. The concluding chapters put forward proposals to deal with collective action problems associated with nuclear accidents and Arctic shipping and explores the processes through which these accords might be negotiated.

The final section assesses the contributions and future status of regime analysis both as a field of study and as a guide to policy makers. In both areas, Young offers qualified optimism. With respect to the former, regime analysis has provided an opportunity for "reintegrating the subfields of international politics, economic, law, and organization" (p. 194). To maintain its durability, however, more work needs to be done in going beyond the study of regime formation and change to the role regimes play as determinants of collective outcomes (p. 206). With respect to the latter aim, Young examines the constraints on attempts to develop socially desirable global regimes and suggests a variety of strategies by which these constraints can be overcome. The importance of devising such strategies will be magnified in the years ahead as environmental and other nontraditional issues figure more prominently on the foreign policy agenda. For those statesmen who will have to think and act in terms of interest defined as cooperation rather than power, this volume will provide a useful guide for action.

KENNETH A. RODMAN

Colby College

Erratum

Making Mondragon: The Growth and Dynamics of the Worker Cooperative Complex, by William Foote Whyte and Kathleen King Whyte (March 1990, pp. 351-352). On page 352 the reviewer's name should read "Kenneth Hoover."

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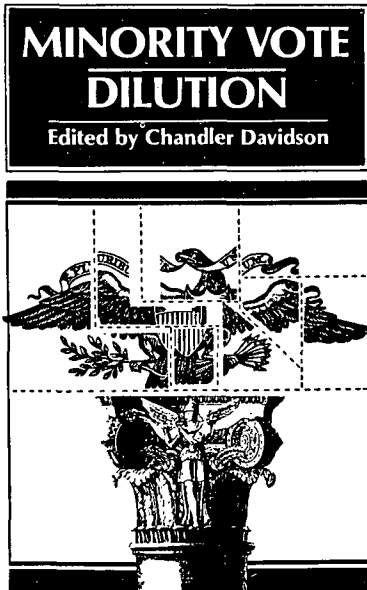
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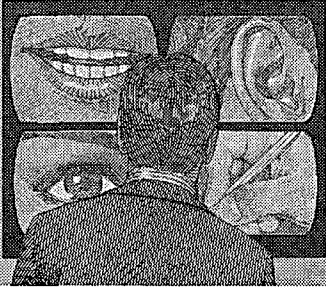
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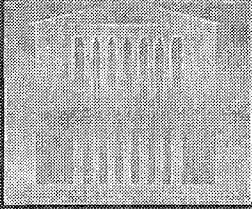
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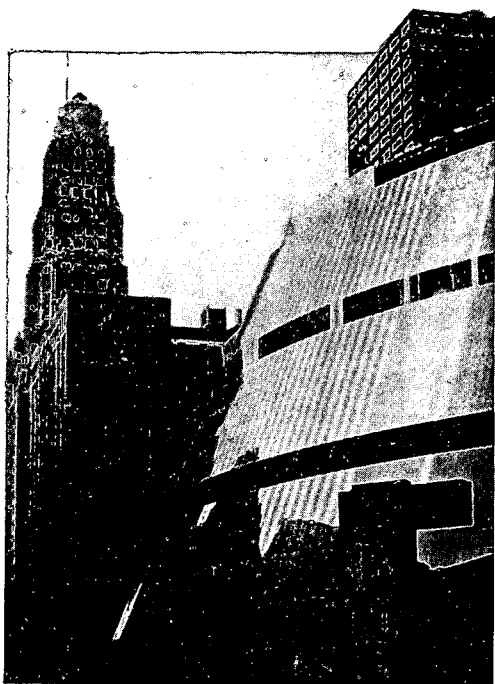
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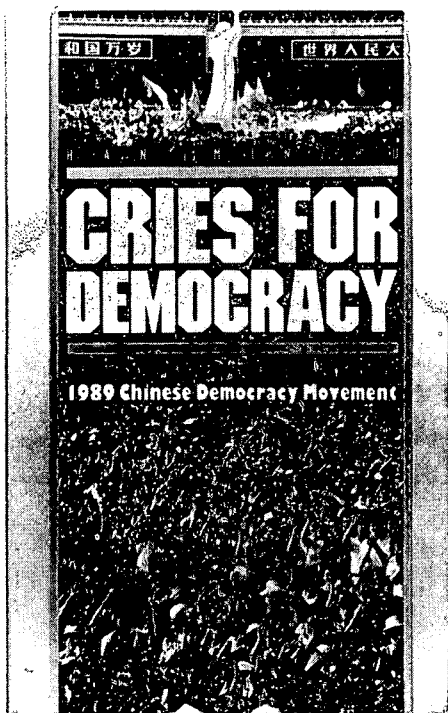
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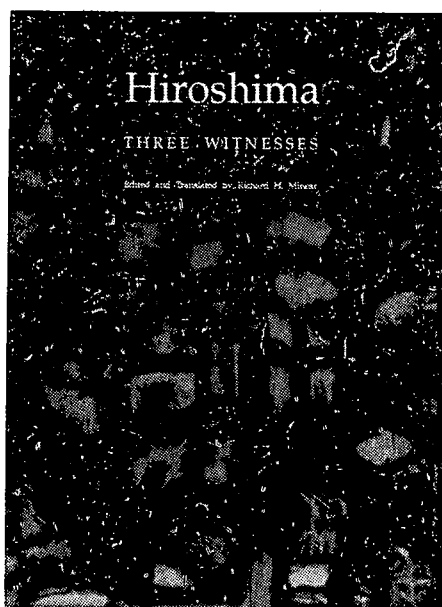
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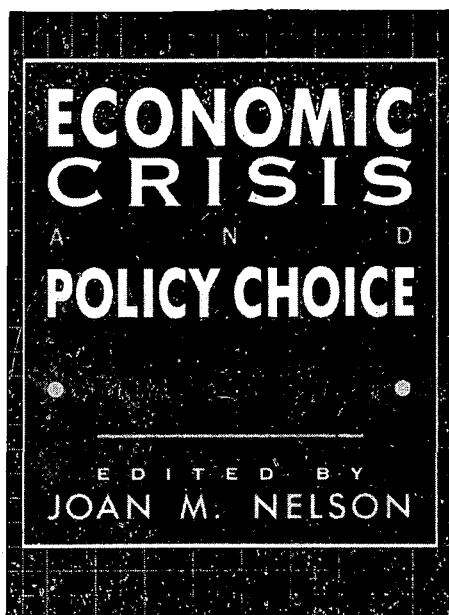
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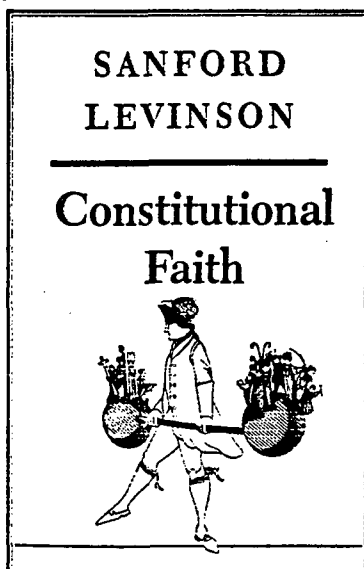
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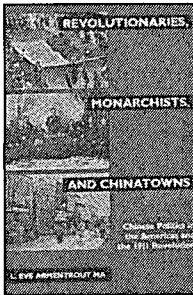
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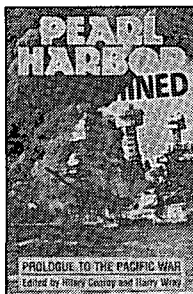
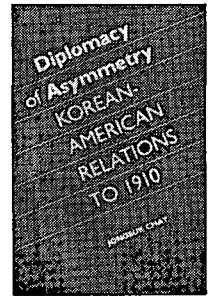


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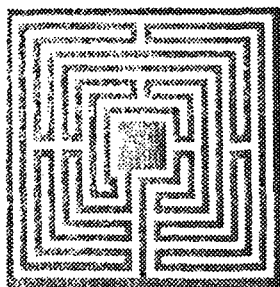
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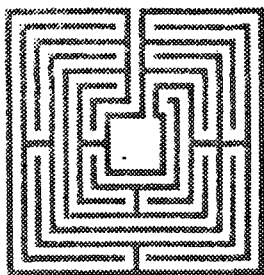
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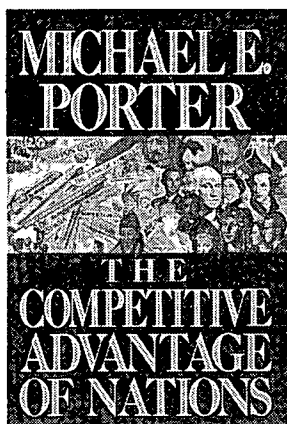
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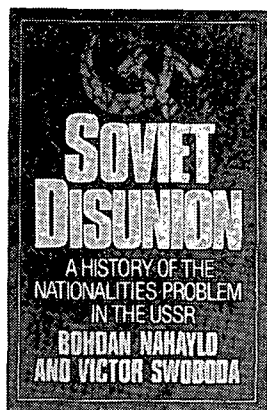
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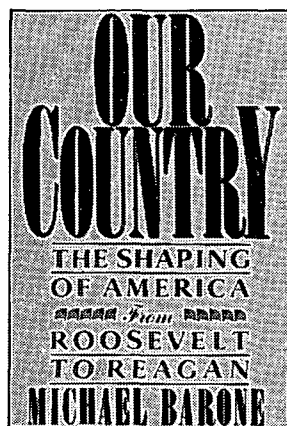
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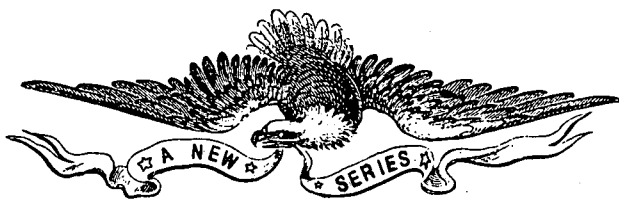
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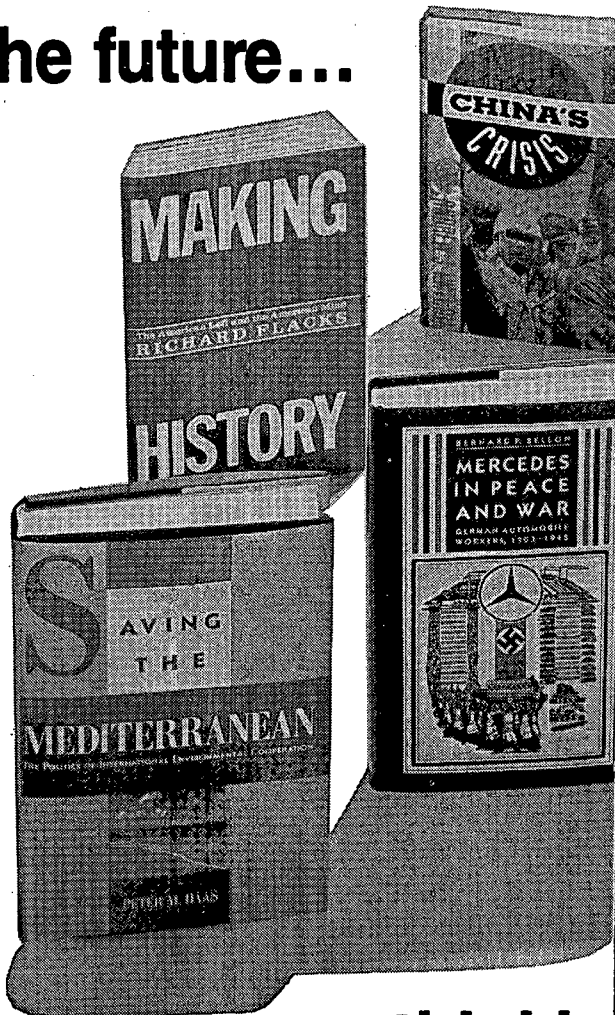
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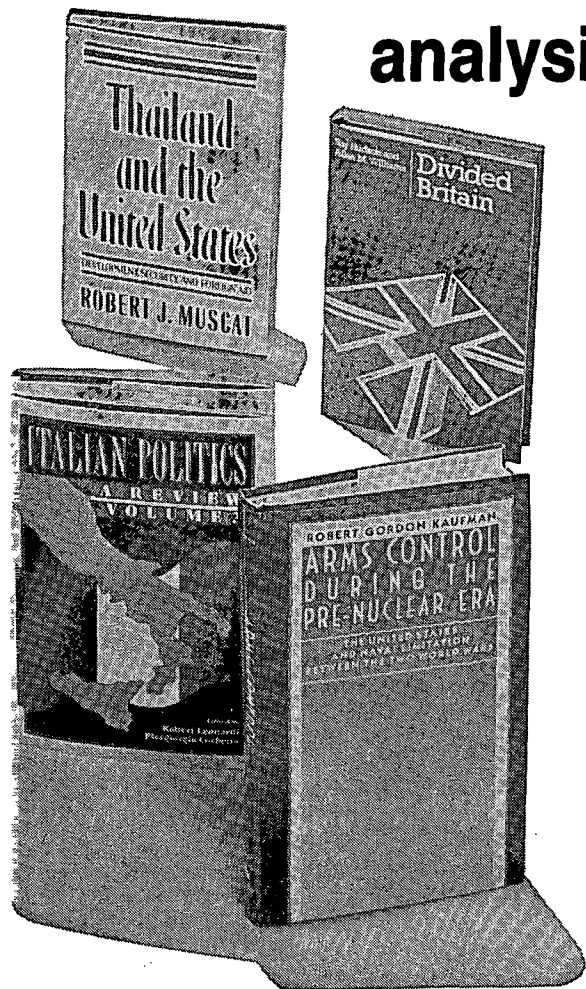
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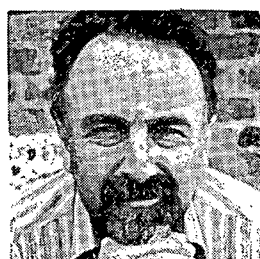
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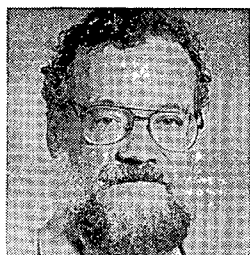
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